SCIENCE

EDITORIAL COMMITTEE: S. NEWCOMB, Mathematics; R. S. WOODWARD, Mechanics; E. C. PICKERING, Astronomy; T. C. MENDENHALL, Physics; R. H. THURSTON, Engineering; IRA REMSEN, Chemistry; J. LE CONTE, Geology; W. M. DAVIS, Physiography; O. C. MARSH, Paleontology; W. K. BROOKS, C. HART MERRIAM, Zoology; S. H. SCUDDER, Entomology; N. L. BRITTON, Botany; HENRY F. OSBORN, General Biology; H. P. BOWDITCH, Physiology; J. S. BILLINGS, Hygiene; J. MCKEEN CATTELL, Psychology; DANIEL G. BRINTON, J. W. POWELL, Anthropology.

FRIDAY, DECEMBER 18, 1896.

CONTENTS:

The Life and Work of Dr. Gould: SETH C. CHANDLER 885

The Influence of Light on the Discharge of Electrified Bodies: ERNEST MERRITT 890

The Limitation of the Comparative Method in Anthropology: FRANZ BOAS 901

Princeton in the Nation's Service: WOODROW WILSON 908

Current Notes on Physiography:—
Recent United States Geologic Folios; Tides of the Bay of Fundy; Hölzel's Geographische Charakterbilder; The Wandering of Lake Nor: W. M. DAVIS. 910

Current Notes on Anthropology:—
The Indians of the Northwest Coast; The Temple of Tepoztlan; Ancient and Modern Umbrian Skulls: D. G. BRINTON 912

Notes on Inorganic Chemistry: J. L. H. 913

Scientific Notes and News 914

University and Educational News 917

Discussion and Correspondence:—
X-Rays: M. CAREY LEA. *Reply to Professor Halsted:* THOMAS S. FISKE. *A Question of Classification:* ROBT. T. HILL. *Professor Woodrow Wilson's Address at the Princeton Sesquicentennial Celebration:* J. MCKEEN CATTELL 917

Scientific Literature:—
Packard's Monograph of the Bombycine Moths of North America: VERNON L. KELLOGG. *Brush's Determinative Mineralogy and Blowpipe Analysis:* J. H. PRATT 923

Societies and Academies:—
Entomological Society of Washington: L. O. HOWARD. *Anthropological Society of Washington:* J. H. MCCORMICK. *Geological Society of Washington:* W. F. MORSELL. *Biological Society of Washington:* F. A. LUCAS. *Academy of Natural Sciences of Philadelphia:* EDW. J. NOLAN. *Northwestern University Science Club:* A. R. CROOK ... 925

MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison-on-Hudson, N. Y.

THE LIFE AND WORK OF DR. GOULD.

SOMEWHAT more than a week has passed since the sods were placed over the honored grave of Dr. Gould, yet even in the busy haste of our American life there are many who will welcome an appreciative account of the life and achievements of this eminent astronomer. So many notices of him were printed in the daily journals, immediately after the community was apprized of its sad loss, that it is unnecessary here to recount the details of his work. But it may be of service to give, in right perspective, a just idea of the magnitude and character of his contributions to astronomy, to speak of the purpose and importance of what he accomplished for science, and of the nature of his strong personal influence in its advancement.

Considered apart from the great things he accomplished, the first thing that strikes us about his career is the intimate way in which it is bound up with the history of his beloved science on both sides of the Atlantic, and the unique position he held, as illustrated by the number and extent of his personal alliances. It is scarcely possible to realize that he was the friend of von Humboldt, then in his 77th year; that it was due to the friendly interest of this great man, indeed, that he became the pupil, friend and intimate in the household of the great master of modern astronomy, Gauss, then in his 70th year; that he was the

pupil of the illustrious Encke, of the elder Struve, of Peters and Hansen and Argelander, and the life-long friend of these revered founders of modern astronomy. His correspondence with them after his return to his native land testifies to the mutual confidence that characterized these memorable associations. Following these, in the next generation, come Winnecke, Schönfeld, Auwers and a host of others, which to us are great names, but to Gould were fellow-pupils and associates. Of the same epoch, on this side the water, we have Bache, Peirce, Walker, Hubbard, Coffin, Chauvenet and Winlock. Then come the two existing generations of astronomers, bringing friendships and acquaintances wide as the world itself, extending his relations to the youngest workers in the science. With such universal and intimate connection with the personal forces operating to advance astronomy in all lands, with his intense patriotism, with his positive intellectual and moral traits, he could not fail to exercise a powerful moulding influence upon the development of American astronomy, in correcting wrong tendencies and establishing right standards. It was to this end, in preference to the satisfaction of ambition through what he could accomplish by his personal contributions to the fund of knowledge, that his most earnest efforts, from the beginning of his career, were directed. In one of his letters to von Humboldt, in 1850, after speaking of the dependent condition at that time of science here, its self-distrust and intellectual timidity, he says: "This I knew before returning home, but realize it now, for the first time, to its full extent. Therefore it is that I dedicate my whole efforts, not to the attainment of any reputation for myself, but to serving, to the utmost of my ability, the science of my country, or rather, as my friend Mr. Agassiz tells me to say, science in my country." And to Encke he says, speaking of the establishment of the *Astronomical Journal*, and his fond hopes in its agency as a means of raising the astronomy of America to its proper position: "Though the labor of supporting it will prevent me from working as I otherwise should for the advancement of my own reputation, still the consciousness that I may render now a still greater service to science reconciles me to the abandonment of a good deal of personal ambition." The same spirit breathes throughout his correspondence with Gauss, Schumacher and other leaders abroad, to whom he was wont to confide his projects and aspirations, and who sympathized with and counseeld him in return. His letter books are rich with similar illustrations. They form, indeed, a treasure house of information relating to the inner history of the beginnings of astronomy in the United States. How humble these beginnings were few realize. Astronomy here has made such wonderful strides within a few decades past that it is hard to believe that, with a few brilliant exceptions, its record about the year 1840 was practically blank. We had then no standing among the scientific nations. The soil for its growth was not indeed sterile, but its cultivation had scarcely begun. If the right seed had not been sown we should have had occasion to deplore to-day the same stunted crop which, even in some countries of far older civilization than ours, stands in place of the rich harvest that might have been gathered under more favoring influences. And this brings us to another remarkable feature of Gould's career. If we were asked to place the finger upon the one epoch marking the birth or regeneration of American astronomy, we should feel inclined to name the date, July, 1845, when Gould placed his foot upon the steamer from Boston with the avowed and definite purpose to devote himself to a life of purely scientific research. Up to that time the instance of a man doing this as

his only aim, while unassured of a professor's chair or similar appointment, and not as a means of livelihood, was in this country absolutely unknown. With a steadiness of purpose singular in so young a man, he pursued diligently the opportunity he had himself created—a year each at Berlin and Göttingen, and shorter periods at Altona, Gotha, Greenwich and Paris—and returned home, full of early honors and flushed with lofty hopes and honorable ambitions.

From this point Dr. Gould's life became one of incessant activity, impressing its mark in many ways upon the intellectual life of the community; but the line of intensest force naturally took the direction of his own beloved science, to which he communicated an impulse not measured merely by what he accomplished for it by his direct investigations, great as that is, but also by the force which always emanates from so earnest a nature as his. He inspired a new breath into American astronomy. The new atmosphere which he brought with him from Germany, where he had caught the spirit of the great masters under whom he studied, became gradually transfused upon this side the sea. His enthusiasm for the introduction of better means and methods of research was caught by his compatriots, their courage to regenerate our science was sustained, and transmitted through various channels to the next and to the present generation. Thus we may say, without fear of being controverted, that American astronomy to-day is a different thing from what it would have been without Gould's predominant influence, deep and quiet but strong, to upbuild it and to free it from the clumsiness and imperfections which still impede it even in some of the otherwise most enlightened nations of the world. It is under his leadership that American astronomy has climbed to where it looks with steady and level eye upon that of Germany, which occupies perhaps a larger but not a loftier plain. It is his example which will stimulate it, in its upward course during the new century nearly upon us, to attain first, in the friendly and honorable national rivalry, the heights commanding all. We cannot presume to say how far the good fortune which has secured our astronomy so exalted a place has been shared by other branches of physical or of natural science in our land; or, if so, whether it can be traced to any similar instrumentality. But there can be no doubt that, so far as astronomy is concerned, this enviable position has been reached, and that among the personal influences contributing to that result Gould's may be justly regarded as preeminent.

Let us now glance at Dr. Gould's more prominent labors, passing by his earlier important investigations in applied theoretical and in practical astronomy, as well as his numerous and valuable contributions to the literature of science, education and other departments of thought, which we find scattered through the long range of his career.

In 1852 he was appointed to take charge of the longitude determinations of the Coast Survey. He organized, developed and extended his service, retiring in 1867. Meanwhile, in 1855, he became Director of the Dudley Observatory in Albany, equipped and organized the institution, and carried it on without remuneration and at his private expense. He left it in 1859, after a severe struggle to preserve the institution for purposes of scientific investigation.

In 1859 he published his discussion of the places and proper motions of circumpolar stars, for use as standards in the Coast Survey. These, as revised by him in 1861, together with his similar list of clock stars, were adopted as the standards for the American Ephemeris, and, as to the circumpolars, remain in such use to this day. In

1866 he published his reduction of D'Agelet's observations. About the same time he performed a similar service for the greater part of the observations made at the United States Naval Observatory since its establishment, as he had done also several years previously for the expedition to Chili to determine the solar parallax. In 1866 he planned and executed the work of establishing, by the Atlantic cable, the relation in longitude between European and American stations, involving, as a part, interesting researches on the velocity of the galvanic current in submarine cables, similar to those he had already made on land lines.

As actuary of the United States Sanitary Commission, he conducted, and published in a large volume, extensive and important researches upon Military and Anthropological Statistics and the Distribution of Population. About the same time he undertook the reduction of Rutherfurd's photographs of the *Pleiades*. The results, partially published in 1866, were submitted completely, in an elaborate memoir, to the National Academy in 1870, together with a second memoir on the *Præsepe*. He was, indeed, a pioneer in the utilization of photography for exact astronomical measurement. About 1864 he built an observatory in Cambridge, equipped with an 8-ft. transit instrument, and, until 1867, carried on a determination of the right ascensions of all the stars to the tenth magnitude within one degree of the pole. This work was completely reduced, but the discussion and publication were postponed by his removal to Cordoba.

In 1865 he became intensely impressed with a desire to explore the southern celestial hemisphere. The opportunity to do so soon came. This project assumed at first the form of a private astronomical expedition, for which his friends in Boston had promised the pecuniary means; but, under the enthusiastic support of Mr. Sarmiento, at first as Argentine Minister to this country, and later as President of that Republic, it rapidly broadened, and finally led to the establishment, by Dr. Gould, of a permanent National Observatory at Cordoba. This marks an epoch in modern astronomy, the equalization of our knowledge of the two celestial hemispheres. The institution and its work form an impressive monument to his memory.

It is impossible, in brief space, to describe or characterize the marvelous work here undertaken and so faultlessly pushed to completion by Dr. Gould, during the fifteen years of self-imposed exile from his native land, with unfaltering devotion and energy, in the face of difficulty and domestic bereavement. The work on the uranography of the southern heavens was finished in 1874, and was published under the title of the *Uranometria Argentina*, which will remain a classic for all time. The zone-observations of the stars between 23° and 80° south declination, which were the original and always the dominant object of the enterprise, were begun in 1872, substantially completed in 1877, and revised in 1882–83. This work was embodied in the Zone-Catalogues containing 73,160 stars, which appeared in 1884. Parallel with this, and almost overshadowing it in importance, was carried on the independent series of meridian circle observations for the General Catalogue of 32,448 stars, completed in 1885. Dr. Gould, besides, left the manuscript of the remainder of his series of fifteen volumes, not then published—containing the observations and the annual catalogues, incorporated in the General Catalogue—complete to the minutest detail, ready for the printer. These have since appeared from time to time; the last volume, rounding out his work, reaching Cambridge but a few hours before his death.

Another part of the work for the Cordoba Observatory, planned by Dr. Gould as a fitting extension of it, was a *Durchmusterung*

of the southern sky. For this, indeed, he had provided the instrumental means and trained the assistants, it being his purpose to be ready to begin it at any time in case of unforeseen delay or accident to the other work. On leaving Cordoba he confined it to the care of Dr. Thome and Mr. Tucker, who have since so worthily conducted it.

Dr. Gould also established, under the auspices of the Argentine government, a meteorological service, second in extent, it is believed, only to that of the United States. Upon leaving South America he intrusted this charge to the hands of his worthy successor, Walter G. Davis.

The earliest to recognize and demonstrate the capabilities of photography to render service to the astronomy of precision, Dr. Gould, by his experience with the Rutherfurd plates of the *Pleiades* and the *Præsepe*, was incited to arrange to carry forward at Cordoba, on an extensive scale, a similar work upon the southern stellar clusters. His other labors there were so onerous that he confined his attention to securing plates suitable for precise measurement. Of these he accumulated about 1,400, and brought them home with him for measurement and reduction. Without permitting himself a well-earned retirement, he turned at once, tirelessly, to this labor, which has been the principal occupation of the last ten years of his life. This is substantially complete, and will be given to the world as it came from his hand.

Dr. Gould had an enthusiasm for the advancement of his beloved science far wider than the limits of what he could by personal investigation accomplish. Early in his career he keenly realized that astronomy had reached a stage of development in America which entitled it to a higher claim than had yet been accorded to it, and that a journal worthily supporting the dignity of a pure science would have very great influence upon its future progress. Accordingly, without ostentation, he established the *Astronomical Journal* in November, 1849, offering it to the use of astronomers, for the publication exclusively of original investigations. He edited and supported it until, at the end of the sixth volume, in 1861, its issue was suspended, first by the war for the preservation of the Union, afterward by his absence in Cordoba. A long nurtured hope was realized when he was enabled, in 1885, to resume its publication and to continue it, at the rate of nearly one volume annually, to the present time. Of all the great enterprises of his life, this is the one which he has most cherished. With careful forethought, he has made due provision for its continuance.

Dr. Gould was born in Boston, September 27, 1824. He entered the Boston Latin School in 1836, and graduated from Harvard College, with high distinction in classical as well as in mathematical and physical studies, in 1844.

He married, in 1861, Mary Apthorp Quincy, daughter of the Hon. Josiah Quincy. She died in 1883. Her sympathy with and influence upon his life-work may be most reverently spoken of by recalling the lines of his dedication of the Zone Catalogue:

"This Catalogue of Southern Stars, the fruit of nearly thirteen years of assiduous toil, is dedicated to the beloved and honored memory of Mary Apthorp Quincy Gould, to whose approval and unselfish encouragement the original undertaking was due, by whose sympathy, self-sacrifice and practical assistance its execution was made possible, who bravely endured privation, exile and afflictive bereavement that it might be worthily finished, but who has not seen its completion."

Dr. Gould received the degree of Ph.D. from Göttingen in 1848, and that of LL.D. from Harvard in 1885, and from Columbia in 1887. During his illustrious career he

was the recipient of the highest honors that Europe has to bestow, to an extent scarcely vouchsafed to any other American. A few only will be named here: Mem. Roy. Soc. (London); For. Assoc. Roy. Astr. Soc. (London); Cor. Mem. Acad. Sci. (Institut de France); Acad. Imp. Sci. (St. Petersburg); Kön. Akad. Wiss. (Berlin); Kön. Ges. Wiss. (Göttingen); Kais. Akad. Wiss. (Vienna); Bur. d. Long. (Paris). He was also knighted, of the Order Pour le Merite, by the German Empire, a distinction never given to any other American and exceedingly rare even in Europe.

Seth C. Chandler.

Cambridge, Mass.

THE INFLUENCE OF LIGHT ON THE DISCHARGE OF ELECTRIFIED BODIES. II.

CONNECTION BETWEEN PHOTO-ELECTRIC SENSITIVENESS AND ABSORBING POWER. WORK OF STOLETOW, HALLWACHS, ETC.

58. The fact that water is an inactive substance as regards the negative discharge was shown by Bichat and Blondlot* by means of an apparatus similar to that used by Righi. Instead of a metal plate a sheet of glass was used, over which a stream of water was allowed to flow. In front of this was placed a wire gauze. It was impossible to obtain any current between water and gauze by illumination, even when 80 cells were used. The authors point out that water is transparent to the effective rays, as first shown by Hertz.

59. Stoletow† used a method similar to that of Bichat and Blondlot with various colored liquids, such as solutions of Fuchsin, Eosin and Fluorescin in ammonia. He came to the conclusion that the effect was always greatest in those liquids which were *capable of absorbing the active rays.*

60. This conclusion was in the main confirmed by Hallwachs* by more reliable and systematic methods. The liquids to be tested were placed in a rather large watch glass, and were illuminated by an arc light placed vertically above the surface. A screen of quartz or gypsum was placed beneath the lamp to prevent disturbance from carbon particles. Connection was made between the liquid and the electrometer by a platinum wire, and the effect of illumination was measured by the rate of dissipation a negative charge on the liquid surface. Some of the results are given below:

Aqueous solutions of Fuchsin, Cyanin	as sensitive as metals
Aqueous solutions of KNO_3, Eosin, Haematoxylin, Aniline	less sensitive
Water; Solutions of Chromic acid, Potassium permanganate, $Co(NO_3)_2$, KNO_3, KBr; Acetone, Amylacetate	no effect

61. Some trouble was experienced on account of the irregularity of the arc lamp. In order to be able to obtain at any time a measure of its intensity, a piece of copper, which had been oxydized by being brought to a red heat, was kept at a fixed distance from the arc, and the rate of negative discharge from its surface measured from time to time. Such a surface is much less sensitive than one that is polished and clean, but it appears also to be more permanent.

62. Hallwachs gives one series of observations showing the influence of the concentration of the solution. It appears that the intensity of the effect increases less rapidly than the concentration.

63. A consideration of the results showed that all the liquids which were sensitive to the influence possessed a strong absorbing power for ultra-violet rays. The connection between absorption and sensitiveness for the effect does not, however, appear to be a

* C. R. 106, p. 1349. Beibl. 12, 605.
† C. R. 106, p. 1593. Beibl. 12, 723.

* Wied. Ann. 37, p. 666.

simple one. An aqueous solution of fuchsin, for example, is very senitive to the action of the ultra-violet rays ; but an alcoholic solution of the same concentration is entirely inactive. Yet the same rays are absorbed by the two solutions, the absorption being more marked with the alcoholic solution. A cell containing the latter, when placed in the path of the rays, destroyed all action on the aqueous solution. The author suggests that the case is analogous to such as occur with fluorescent substances, where the solvent has great influence upon the intensity of the fluorescence.

64. Hallwachs next made an attempt to investigate the effect of different wavelengths in the neighborhood of ultra-violet absorption bands. A solution of fuchsin showed a strong band between $\lambda = .250\,\mu$ and $\lambda = .275\,\mu$. The solution was tested in a spectrum formed with a quartz prism. But the effect was so weak as to make reliable results impossible.

An approximation to monochromatic rays was obtained by using suitable absorbing media. But it then appeared that the most active rays were in the *extreme* ultra-violet, beyond the absorption bands. The effect in the neighborhood of the bands was too small to enable any conclusion to be drawn.

65. During his experiments Hallwachs found that some of the less active liquids showed some slight effect when *positively* charged. He was inclined to believe, however, that this effect was accidental, and due to the discharge of the induced *negative* electricity on neighboring bodies.

LATER WORK OF WIEDEMANN AND EBERT.

66. At about this time further investigations were undertaken by Wiedemann and Ebert* upon the spark discharge as influenced by ultra-violet rays. The method was practically the same as that first used by these writers.*

Various metals were first investigated. The electrodes were made of the same form in each case, and great care was shown in obtaining the same intensity and quality of illumination. It was found that Pt was by far the most sensitive of all the metals tried. A telephone, placed in series with the spark gap, showed an especially noticeable change in the character and pitch of the sound when a spark gap of about 2 to 3 mm. was illuminated. The effect in other metals is very much less intense.

In the order of decreasing sensitiveness, the metals are as follows: Zn, Cu, Fe, Al, Pd, Ag.

67. The sparking distance most favorable for showing the effect was found to be different with different metals. The change in the appearance of the spark, under the influence of ultra-violet rays, is discussed in Wiedemann and Ebert's paper at some length (p. 213 l. c.).

68. Experiments with liquids showed that strongly absorbing solutions were most active. The most sensitive liquid was found to be a solution of Nigrosin. Water showed only a very slight effect, which, however, was greatly increased by impurities.

69. At low pressures the effect of illumination was practically *nil.* (But a *slight* reduction of pressure increases the effect. See Hertz, and Wiedemann and Ebert 1st paper.) "As soon as the pressure conditions are such as to cause a considerable development of cathode rays, the influence of illumination can no longer be noticed" (p. 217).

70. Wiedemann & Ebert call attention to the fact that, since their experiments were made upon disruptive discharges, some caution must be used in applying these results to cases of continuous discharge, such as those investigated by Hall-

* Wied. Ann. 35, p. 209. Before Hallwachs and Lenard and Wolf.

* Wied. Ann. 33, p. 211. See §§ 11–16.

wachs, Righi, etc. The difference in the nature of the discharge may be a sufficient explanation of apparent contradictions.

71. In discussing the various observations upon the phenomenon in question, E. Wiedemann * is inclined to favor the view that the effect is due to the absorption of ultra-violet rays. He points out that the absorption of rays by resonance brings the absorbing molecules temporarily into the same condition of vibration which they would have if heated sufficiently to send out the same kind of rays as those absorbed, *e. g.*, fluorescence. The rapid and violent vibrations produced in the surface layers of a body by ultra-violet absorption will produce by collisions increased velocity of translation among the molecules, *i. e.*, a rise of temperature. Although the actual temperature rise will be small, yet individual molecules may receive very high velocities, and, being close to the surface, may escape. Convection discharge will therefore be accelerated by the action of ultra-violet light. Note that this explanation fails to give any reason for the selective discharge of negative electricity, unless there is some reason why the particles take on a negative charge by preference. It also fails to explain the fact that the action of ultra-violet rays is much more intense when CO_2 is used instead of air. †

PHOTO-ELECTRIC EFFECT OF SUNLIGHT. BEHAVIOR OF SODIUM AND POTASSIUM.

72. It will be remembered that all of the early observers failed to detect any action produced by sunlight, either on the spark discharge or upon the continuous dissipation of negative electricity. In 1889, however, Hoor claims to have detected some slight effect.* During the same year Nodon† found that insulated conductors of carbon or metal became positively charged when illuminated by sunlight. And almost simultaneous with Nodon, Elster and Geitel ‡ observed both the discharge of negative electricity and the development of a positive charge. A freshly polished zinc plate was found to acquire a positive potential of 2.5 volts. Even diffused daylight produced a measurable effect. Mg and Al were found to be more sensitive than Zn.

73. Based upon this action of sunlight upon the atmosphere and the surface of the earth, Arrhenius § developed in the same year an interesting theory to explain atmospheric electricity, the aurora, etc.

74. The first article of Elster and Geitel is merely a brief note, hurriedly published, probably in order to secure priority. Two months later a full description of their experiments appear in the Annalen.‖ The first results were obtained with zinc. A shallow dish 20 cm. in diameter was set up (insulated) in the open air, and was connected with a sensitive electrometer in an adjacent building. The dish was surrounded by a metal case, the top of which could be removed by means of a cord running into the laboratory. Above the sur-

* Wied. Ann. 35, p. 257.

† Wiedemann and Ebert. Wied. Ann. 33, p. 240. This result, first obtained for spark discharges, was found to be also true in the case of a continuous discharge. See Stoletow, C. R. 107, p. 91. Beibl. 12, 723. Observations by Breisig, in which zinc was used as the metal and a lamp flame as the source of illumination, fail to confirm Stoletow's observations. [See Beibl. 17, 60, 1891.] But on the other hand, Breisig's results are contradicted by Cantor. [Beibl. 19, 583.]

* Exner's Repertorium, 25, p. 91; Beibl. 13, 731. Righi, however, (Exner's Repertorium 25, p. 380. Beibl. 14, 68) contradicts the results of Hoor, and states that he has 'never succeeded in obtaining the slightest trace of an effect from sunlight.' Elster and Geitel (Wied. Ann. 38, p. 498) were also unable to obtain any action with the substances used by Hoor.

† C. R. 109, p. 219. Beibl. 13, 976.

‡ Wied. Ann. 38, p. 40. Phil. Mag. 28, p. 427. (Abst.)

§ Meteorologische Zeitsch. 5, pp. 297 and 348. Beibl. 13, 328.

‖ Wied. Ann. 38, p. 497.

face of the dish, and only a few millimeters distant, was placed a grounded piece of wire gauze, which served to protect the dish from the influence of the atmospheric electricity. With this apparatus a marked effect could be observed both with direct and diffused sunlight. Wires of Zn, Mg, or Al carefully cleaned and attached to an Exner electroscope were found to give a simple and sensitive portable apparatus.

75. Elster and Geitel appear to have been at first interested in the phenomenon chiefly on account of its important bearing upon meteorology, atmospheric electricity, etc. Much has in fact been written on this phase of the subject, which can only be mentioned here. They were anxious to obtain some instrument by which the actinic intensity of sunlight could be measured from hour to hour and day by day. Since the effect is influenced so greatly by slight changes in the character of the sensitive surface, it was difficult to find any active substance which would remain permanent. *Amalgamated zinc* was however found to fulfill the requirements, being much more permanent than pure zinc, and also more sensitive.

76. Elster and Geitel called attention to the fact that only *electro-positive* metals appear to possess 'actino-electric' sensitiveness.* It seemed therefore natural to expect that metals that are even more positive than Zn, Al and Mg, should show the effect in more marked degree. K and Na were tested, but, on account of the rapid oxydation of the surface, no action could be observed. The same trouble was met with in the case of solid amalgams of K and Na. By using a dilute solution of K or Na in mercury, and allowing the liquid to flow continuously from an aperture, a clean surface could however be maintained, and under these circumstances the effect of light upon a negative charge was very marked. With positive electricity there was no action. Pure mercury was entirely inactive.

77. The most sensitive metals as regards the action of sunlight were, in the order of sensitiveness:

K, Na, Al, Zn, Sn.

This series is exactly the same as the Volta contact series.

78. Sunlight produced no effect on unamalgamated but clean surfaces of

Sn, Cd, Pb, Cu, Brass, Fe, C, Pt.

79. From a meteorological standpoint experiments with surfaces of water appear of considerable interest. The zinc dish described above was therefore filled (1) with pure rain water; (2) with: hot water (rapidly evaporating); (3) with salt water. No action could be detected from sunlight in any case. If any action exists it must be hundreds of times smaller than that observed with zinc.

80. In the course of these experiments Elster and Geitel obtained a noticeable effect with a powder used in making luminous paint (Balmain's Leuchtfarbe).

81. The action in the case of sunlight seems to depend upon somewhat longer waves than those of the extreme ultra-violet. Glass and mica, for example, do not stop the effect completely.

82. Elster and Geitel describe, at the close of their article, convenient forms of apparatus for showing the fundamental phenomena.* Lecture experiments are also suggested by Hallwachs.†

83. A method of conveniently using the highly sensitive sodium amalgam without deterioration of its surface was devised soon after by Elster and Geitel.‡ The amalgam was prepared and kept in a vacuum

*See § 34.

* An improved form of apparatus for this purpose is described in Wied. Ann. 42, p. 564.

† Wied. Ann. 41, p. 161.

‡ Wied. Ann. 41, p. 161.

tube, the pressure being in the neighborhood of 1 mm. Under these conditions the surface remained permanently unaltered. Such an active electric cell was extremely sensitive. Diffuse light was effective even through the glass sides of the tube; no quartz window or similar arrangement was at all necessary. The surface is sensitive even to rather long waves, for a slight effect was observed with the sodium flame.

84. In 1891 Elster and Geitel* again took up the investigation of the actino-electric behavior of sodium and potassium. Instead of amalgams, the clean unoxydized surfaces of the metals themselves were used. Such surfaces were prepared in vacuo by a method described at length in the article cited. The apparatus and connections are shown diagrammatically in Fig. 2. The metal, M, was kept perma-

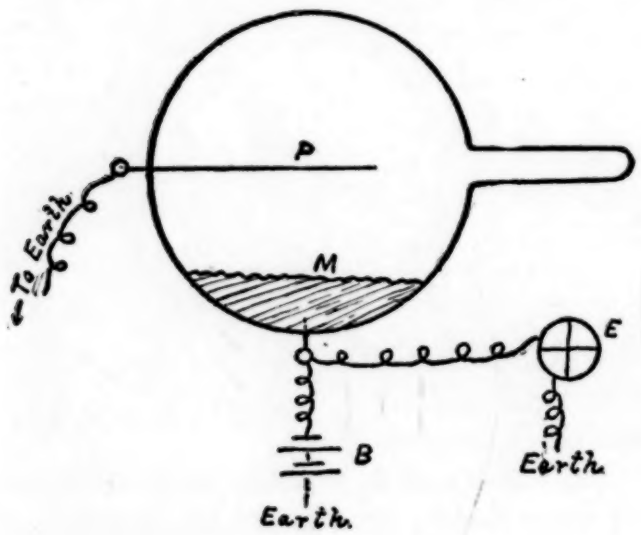

FIG. 2.

nently connected with the negative pole of a 'Zamboni' battery, B (a dry battery. See Wied. Elect. I.: 272), the positive pole of the battery being grounded. The potential of the negative pole was then measured by an Exner electroscope. (E in figure.) A platinum electrode, P, above the surface of the metal was kept grounded and served to carry away the charge dissipated from M.

* Wied. Ann. 43, p. 225.

85. Surfaces of pure potassium were found excessively sensitive. It was impossible to maintain a negative charge at all in a room lighted by daylight. An oil lamp 10 cm. distance reduced the potential from 212 volts to zero. Even at a distance of 6 m. its effect was noticeable. Magnesium light at the latter distance reduced the potential 60%. Even the rays from a Bunsen burner were effective, as was also the fluorescent light from luminous paint. A noticeable effect was observed with moonlight.

86. The effect of various absorbing media was next studied. Finally, the apparatus was placed in the solar spectrum formed by a glass prism. The maximum effect was found in the blue, but the influence was noticeable as far as the red, and far into the ultra-violet. It is suggested that this apparatus may be used to advantage in showing (for lecture experiments) the existence of rays beyond the violet.

87. On charging the potassium positively Elster and Geitel in some cases observed what appeared to be an increased rate of dissipation of *positive* electricity.* They show conclusively, however, that this result is due to the action of diffuse rays upon the (now) negative platinum electrode, P (see Fig. 2), and are strongly of the opinion that other observers who have published accounts of an action on positive charges have been misled by some similar disturbance.

88. Pure sodium surfaces were found somewhat less active than potassium. Experiments upon various amalgams gave results showing that the order of sensitiveness is as follows: Rb, K, Na, Li, Mg, Tl, Zn. Pure K and Na are more sensitive than any of the amalgams.

89. After a number of papers dealing with atmospheric electricity and the meteorological aspect of the negative discharge phenomena, Elster and Geitel again took

* l. c., p. 236.

up their study of the alkali metals in 1894.* Cells containing metallic Na, K and Rb were prepared by methods described in earlier papers.† An atmosphere of hydrogen was used in each case, the pressure being reduced to such a value (about $\frac{1}{2}$ to $\frac{1}{3}$ mm.) as would make the apparatus most sensitive. All measurements were made with a galvanometer, current passing between the illuminated surface and a ring of wire placed just in front of it.

90. Rubidium was found much more sensitive than either Na or K. The maximum effect was also further toward the red than in the case of Na and K, as shown by experiments with various absorbing solutions. For Rb the maximum was in the yellow, with a very noticeable effect in the orange and red. With both Na and K the maximum action was in the blue.

91. Elster and Geitel draw the conclusion that "not only does the actino-electric sensitiveness increase with the electro-positive character of the metal, but the electro-positive metals also show an increased tendency to be affected by light of greater wavelength."

92. In this same article‡ observations are also described showing the effect of illumination in helping the development of Hertz vibrations in vacuum tubes. The effect is especially marked when one of the alkali metals is present. These observations are intimately connected with work by Zehnder, Warburg and others on the behavior of Geissler tubes containing sodium. To follow out the matter in detail would require the considerable literature of this subject to be consulted. I shall therefore merely state the conclusion reached, viz: that in the presence of one of the alkali metals the effect of illumination is to render the gas capable of conducting the rapidly oscillating currents from a Hertzian oscillator.* Taken in connection with Arrhenius' discovery that rarified gases conduct electrolytically when illuminated,† this result has an important bearing upon J. J. Thomson's suggested explanation of actino-electric phenomena.‡

*Wied. Ann. 52, p. 433.

†A special mode of procedure was necessary in the case of Rb.

‡ l. c., p. 445.

BEHAVIOR OF FLUORESCENT MINERALS.

93. The theory advanced by Arrhenius, by which many phenomena of atmospheric electricity are explained as a result of the action of sunlight in discharging negative electricity from the earth's surface, seems to require that other substances besides the metals should be acted upon. A number of minerals, woods, etc., were therefore tested by Lampa,§ with negative results. Preliminary experiments by Elster & Geitel‖ also failed to show any action of sunlight upon such substances. The sensitiveness of one non-metallic substance (Balmain's luminous paint) had however already been proved.¶ This substance being phosphorescent, it seemed not improbable that other phosphorescent materials would show the effect. Later experiments by Elster and Geitel** confirm this view. Various artificial phosporescent powders (*e. g.*, the sulphides of Ca, Sr, etc.) were found to show greater or less actino-electric sensitiveness, according as they were more or less strongly phosphorescent.

94. To observe the action of sunlight upon phosphorescing minerals, more sensitive apparatus was needed than that previously used. The arrangement of apparatus is shown in the figure. A dish, D, of oxydized iron was enclosed in a grounded

*See Wied. Ann. 52, p. 454.

† Wied. Ann. 32, p. 545 ; 33, p. 638.

‡ See § § 112, 113, below.

§ Wiener Anz. 1890. Beibl. 15, 384.

‖ Wiener Berichte 99, p. 1008.

¶ Wied Ann. 38, p. 507.

** Wied Ann. 44, p. 722.

method by which the negative charge is dissipated, they are inclined to adopt Righi's original view that the air particles act as carriers for the electricity. Against the explanation of Lenard and Wolf (particles of the body itself carrying charge) they urge the objection that charged dust particles will not be acted on by a magnetic field. (Why not, if in motion?) Elster and Geitel do not believe in the action of the condensed layer of gas (Hoor), but think that by a sort of resonance the ultra-violet rays make it easier for the gas particles in the neighborhood of the surface to receive a charge.

LATER INVESTIGATIONS OF ELSTER AND GEITEL. ACTINO-ELECTRIC EFFECT OF POLARIZED LIGHT.

100. Further actino-electric investigations by Elster and Geitel appear in Wiedemann's Annalen in 1892.* The authors point out that the behavior of an illuminated cathode is similar in many respects to that of an incandescent cathode.† If the analogy holds true, the resistance of a Geissler tube should be diminished by illumination of the cathode, as it is by heating the latter. The observations of Wiedemann and Ebert, however, contradicted this conclusion for low pressures.‡ Elster and Geitel tnerefore repeat these observations, using potassium as a cathode in a tube filled with hydrogen. Illumination of the potassium surface was found very noticeably to diminish the resistance (as shown by using an adjustable spark gap in air, in parallel with the tube). The effect was most marked at low pressures (0.1 mm. to 0.01 mm).

101. This same tube was then used to study the action of a magnetic field upon the actino-electric current. It was found that when the lines of force of the field were perpendicular to the direction of the convection current, the action in restraining the discharge produced by light was greatest. If the lines of force were parallel to the current, the action was either *nil* or else slightly reversed. In these investigations Elster and Geitel found it possible to measure the actino-electric current with a galvanometer, instead of using an electrometer.

102. In 1894 observations were made by Elster and Geitel * upon the actino-electric action of polarized light. Wanka † had already tried to detect a difference in the discharging action of light depending upon the direction of the plane of polarization, but without success. The difficulty seems to have been partly in the character of the surface and partly in the fact that completely polarized ultra-violet rays are not readily obtained. Elster and Geitel avoided the last difficulty by using cells of Na and K, which were sensitive to visible rays. A smooth and approximately plane surface was obtained by using the metals in liquid form. The light used was polarized by means of a nicol.

103. At oblique incidences the actino-electric effect was greatest when the vibrations of the incident light took place in a plane perpendicular to the sensitive surface, (Fresnel's Hypothesis), and least when the vibrations were parallel to the surface. The difference between the maximum and minimum was a function of the angle of incidence, and was found to be greatest for an incidence of about 60°.

104. In a later article‡ Elster and Geitel describe still further experiments on the action of polarized light. Keeping the

* Wied. Ann. 46, p. 281.

† The discharge from incandescent metal surfaces has been investigated by Elster and Geitel and others at considerable length. Most of the work on this subject will be found in Wiedemann's Annalen.—E. M.

‡ Wied. Ann. 35, p. 217.

* Berliner Akad. 6, p. 133. Wied. Ann. 52, p. 440. Abstracted in Phil. Mag. 38, p. 158.

† Mitth. d. Matn. Gesellschaft in Prag., p. 63, 1892.

‡ Wied. Ann. 55, p. 684, 1895.

metal case, S, with an insulated wire leading outside to an electrometer. Just above D was a sheet of iron wire gauze, G. The substance to be tested was placed in the dish, D (in powder form), and the gauze, G,

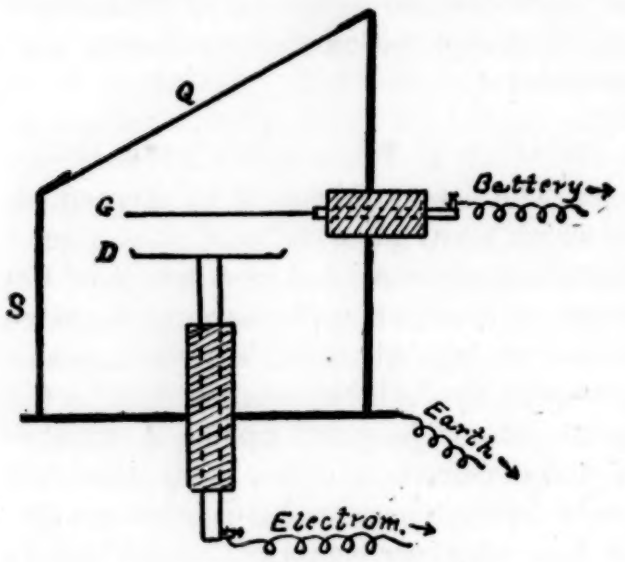

FIG 3.

was kept at a positive potential of 300 volts. The quartz window, Q, being covered by a metal plate, D was grounded for an instant. The ground was then broken and light allowed to shine upon D. As the induced negative charge was dissipated, the electrometer showed an increasing deflection. This arrangement was found much more sensitive than that used before.

95. Among the minerals tested, certain varieties of fluorescing fluorspar were found most sensitive. The most effective rays were not the ultra-violet, but the blue. The sensitiveness of fluorite was not increased by diminished air pressure; and was not destroyed by wetting the surface with water.

96. Fluorite was rendered insensitive by being heated to redness; and its power of phosphorescing was also destroyed by this means. Elster and Geitel suggest that the phosphorescence and actino-electric activity are due to the presence of some organic impurity which is removed by heating.

Various other materials were tested and found to be slightly sensitive. Freshly fractured surfaces were more active than old surfaces.

EFFECT OF A MAGNETIC FIELD ON ACTINO-ELECTRIC PHENOMENA.

97. In the course of his low pressure experiments in 1890,* Righi found that the positive charge acquired under the influence of illumination is lessened when a magnet is so placed as to make the lines of force parallel to the surface. The particles which carry the charge 'appear to meet with opposition in their motion through the magnetic field, so that the actino-electric action is weakened.'

98. The influence of the magnetic field in diminishing the effect of illumination was discovered independently by Elster and Geitel,† who were led to investigate the matter through a consideration of somewhat similar effects in the case of the spontaneous discharge between incandescent and cooler bodies.‡ Amalgamated zinc was used as the active metal and the spark of an induction coil as a source of light. At pressures below 5 mm. the influence of illumination could be in some cases entirely destroyed by a magnetic field. In place of air H, CO_2, and O were also tried. CO_2 was found especially sensitive, both to the actino-electric effect and to the restraining influence of the magnetic field, but much lower pressures were necessary to exhibit the latter action. (Maximum at .005 mm.) These experiments were successfully repeated with daylight, sodium amalgam being used instead of zinc.

99. At the close of the article just cited § Elster and Geitel discuss at some length the probable cause of the phenomena. While accepting some form of convection as the

* Acc. dei Lincei 6, p. 81. Acc. di Bologna 10, p. 85. Beibl. 14, p. 1167.

† Wied. Ann. 41, p. 166. 1890.

‡ Wied. Ann. 37, p. 315; 38, p. 27.

§ l.c., p. 174.

prove Branly's conclusions. The experiments of the latter were repeated as nearly as possible under the original conditions, and indications of a loss of positive electricity were in fact found. But it was in all cases possible to ascribe these results to disturbances, as in Elster and Geitel's previous experiments. And when the experiments were modified so as to more completely remove the sources of disturbance, the apparent loss of positive electricity became insignificant. In view of the fact that the X-rays, whose discharging action is in many respects similar to that of ultra-violet light, are capable of dissipating *both* charges, it seems probable that some effect is also produced by light upon a positive charge. The experiments of Elster and Geitel indicate, however, that this action must be extremely small.

THEORIES PROPOSED.

111. Although it has been shown, I think conclusively, that the dissipation of a negative charge by light is accomplished by convection, the theories heretofore cited do not explain how this convection is brought about; and they also leave unexplained the still more important fact that negative charges only are affected. The theories proposed by J. J. Thomson* and by Elster and Geitel† respectively seem satisfactory, however, in regard to both these difficulties.

112. Thomson bases his explanation of the phenomenon upon the hypothesis of Helmholtz that "bodies attract electricity with different degrees of intensity." This conception was shown by Helmholtz to be able to explain electrification by friction and the differences of potential produced by contact. If the attraction of a metal for positive electricity is greater than that of the dielectric surrounding it, the tendency is for the metal to become positively charged.

* Recent Researches in Elect. and Mag., p. 64.

† W. A. 55, p. 697.

But "when a conductor, which does not disintegrate, is surrounded by air in its normal state, * * * the conductor cannot get charged * * * for the electricity of opposite sign to that which would be left on the conductor has no place to which it can go." "The case is, however, different when the conductor is exposed to the action of ultra-violet light, for then, as Lenard and Wolf's experiments prove, one or both of the following effects must take place: (1) disintegration of the conductor; (2) chemical changes in the gas in the neighborhood of the conductor which put the gas in a state in which it can receive a charge."

113. The first effect mentioned, namely, disintegration of the metal, might be produced by the absorption of the light rays, as explained in § 71. On the other hand, it is quite conceivable that light, and especially the ultra-violet rays, may produce electrolytic dissociation in the gas, and so render the latter capable of taking a charge. This hypothesis is strengthened by the experiments of Arrhenius,* who found that certain gases became conductors when illuminated. That some such change in gases is produced by X-rays is now proven beyond question.

114. The theory suggested by Elster and Geitel depends upon the assumption that the electro-positive character of the metal relatively to the dielectric causes the formation of two charges, viz: A positive charge on the metal, and an equal negative charge in the adjacent portions of the gas. These two charges being extremely close together, their outside effect disappears. When light waves fall upon the surface in such a manner as to give a component electric displacement (electro-magnetic theory) in the direction of the normal, electric oscillations will be set up in the metal. These oscillations may be sufficiently powerful to destroy

* Wied. Ann. 32, p. 545; 33, p. 638; Phil. Mag. 28, p. 75.

angle of incidence constant they investigated the change in the actino-electric current as the polarizing nicol was rotated. Attempts were made to use a cell containing a plane-parallel quartz window, but no cement could be found which would hold and yet not alter chemically the sensitive surface. Cells could, however, be constructed which would remain tight for a short time, and served to check the results obtained with spherical glass cells. In using the latter cells precautions were taken to have the rays from the zircon lamp pass normally through the glass. Only the central portion of the liquid metal surface was used.

105. Indicating the angle through which the nicol was turned from the position of maximum action by x, the actino-electric current was found to be given by the equation:

$$I = A \cos^2 x + B \sin^2 x.$$

This formula agrees with the assumption that the actino-electric effect is proportional to the incident light, but that the proportionality factor is different for light polarized in the plane of incidence and perpendicular thereto.

106. Observations were made at several different angles of incidence (70°, 66°, 40°, 23°). The ratio of A to B was found to depend upon the angle of incidence. Both A and B were small for normal incidence. A increases to a maximum at an incidence of about 60°, and then decreases. B appears to decrease steadily to practically zero at grazing incidence. At 60° the ratio of B to A was found to be 50.

107. Good effects with polarized rays could only be obtained with the smooth, nearly plane surface of liquid K or Na. Solid Na or K always show rather a rough surface and behave in practically the same manner for polarized and unpolarized light.

108. Experiments were tried with amalgamated zinc at ordinary pressures, but here, too, the behavior with polarized rays was scarcely different from that with unpolarized light. The authors think that this is due to the difficulty of obtaining ultra-violet rays completely polarized.

EFFECT OF LIGHT UPON POSITIVELY CHARGED BODIES.

109. When the attempt is made to find a satisfactory explanation of the action of light upon charged bodies, the fact that the action seems to be confined to *negative* charges is of great significance. Several observers have, indeed, found indications of a discharging action in the case of positively charged bodies. Mention has already been made of such indications in the case of the experiments of Hallwachs, Righi, and Elster and Geitel. But these observers were able to show that the apparent dissipation of a positive charge under the influence of ultra-violet rays was in reality a secondary phenomenon, due to the convective discharge from negative bodies in the neighborhood. They were of the opinion that the action of light upon a positive charge, if such action exists at all, is too small to be measured.

110. In contradiction to these conclusions stand the results of numerous experiments by Branly.* Under circumstances where previous observers had found no trace of any action, Branly detected a very rapid dissipation of positive electricity. The lack of a detailed description of the experiments leading to these results makes it impossible to draw any conclusion in regard to their reliability. But the question is of such importance that Elster and Geitel† have recently undertaken a series of experiments intended either to confirm or dis-

* C. R. 110, p. 751, 1890; 110, p. 898; 114, p. 68, 1892; 116, p. 741, 1893; Lumière elect. 41, p. 143, 1891; Jour. de Phys. 2, p. 300, 1893; Abstracts in the Beiblätter.

† Wied. Ann. 57, p. 24, 1896.

at times the positive surface charge, or even to reverse it. The negatively charged particles of the gas will thus be driven away, while others will come to take their places, and in so doing will impart additional positive electricity to the metal. The fact that strongly electro-negative gases, such as H and CO_2, give an especially marked photo-electric action seems to support this view. On the other hand, as is pointed out by Elster and Geitel, this explanation seems to require that all action should cease when the light rays fall normally on the surface, a conclusion which is not borne out by experiment.

CONCLUSION.

It is clear from what precedes that a thoroughly satisfactory explanation of the discharging action of light has not yet been found, and that many questions concerning the phenomenon remain to be settled by further experimental investigation. That interest in the subject is not diminishing is evidenced by the number of papers that have only recently been published. A list is given below of those that have come to my attention too late to enable their results to be included in this article.

Elster and Geitel. The influence of light on the form of the discharge from a Holtz machine. Wied. Ann. 57, p. 401, 1896.

Elster and Geitel. On a photo-electric after effect of cathode rays. Wied. Ann. 59, p. 417. 1896.

Warburg. The action of light on the spark discharge. Wied. Ann. 59, p. 1. 1896.

Klemencic. A lecture experiment to illustrate the mutual influence of two spark gaps. Wied. Ann. 59, p. 63.

Swyngedauw. Action of ultra-violet light upon explosive, static and dynamic potentials. C. R. 122, p. 131, 1896. Beibl. 20, 660.

Dufour, Dutoit and *Hofer.* Dissipation of electricity under the action of light. Arch. de Genéve, 34, p. 294, 1895.

Simon. On the influence of waves of high refrangibility upon the electrical conductivity of rarified gases. Wiener Berichte, 104, p. 565, 1895.

Batelli and *Garbasso.* On the dissipation of electrostatic charges by ultra-violet rays. Nuovo Cimento III., p. 321, 1896.

Sella and *Maiorana.* Action of Röntgen raysand of ultra-violet light on the disruptive discharges in air. Nuovo Cimento III., p. 238, 1896.

The bearing of the phenomena here described upon the question of the nature of the X-rays is doubtless of especial interest at the present time. The fact that the X-rays are capable of producing a rapid dissipation of electricity from charged bodies has been urged in support of the view that these rays are similar in kind to ultra-violet light, but of excessively small wave-length. The argument loses some of its force when we recall the fact that ultra-violet rays are capable of dissipating a *negative* charge only, while the action of the X-rays is practically the same for both charges. This difference between the two phenomena does not in itself offer a fatal objection to the argument, but recent experiments of Batelli and Garbasso (see above reference) indicate a much more fundamental difference between the two effects. It has been found that air which has been exposed to X-rays retains the power of discharging electrified bodies even when no longer exposed to those rays. No indication of such an after effect has been found in the case of ultra-violet light. The work of Batelli and Garbasso was undertaken with the special object of testing this point, and their results show that no trace of an after effect can be detected with the rays from an arc lamp, even when the direct discharging action is as great as that produced by X-rays. If these results are confirmed I think we shall be forced to the conclusion that the discharging action of ultra-violet light differs essentially from the similar effect produced by the Röntgen rays.

Ernest Merritt.

Cornell University.

THE LIMITATIONS OF THE COMPARATIVE METHOD OF ANTHROPOLOGY.*

MODERN Anthropology has discovered the fact that human society has grown and developed everywhere in such a manner that its forms, its opinions and its actions have many fundamental traits in common. This momentous discovery implies that laws exist which govern the development of society, that they are applicable to our society as well as to those of past times and of distant lands; that their knowledge will be a means of understanding the causes furthering and retarding civilization; and that, guided by this knowledge, we may hope to govern our actions so that the greatest benefit to mankind will accrue from them. Since this discovery has been clearly formulated, anthropology has begun to receive that liberal share of public interest which was withheld from it as long as it was believed that it could do no more than record the curious customs and beliefs of strange peoples; or, at best, trace their relationships, and thus elucidate the early migrations of the races of man and the affinities of peoples.

While early investigators concentrated their attention upon this purely historical problem, the tide has now completely turned, so that there are even anthropologists who declare that such investigations belong to the historian, and that anthropological studies must be confined to researches on the laws that govern the growth of society.

A radical change of method has accompanied this change of views. While formerly identities or similarities of culture were considered incontrovertible proof of historical connection, or even of common origin, the new school declines to consider them as such, but interprets them as results of the uniform working of the human mind. The most pronounced adherent of this view in our country is Dr. D. G. Brinton, in Germany the majority of the followers of Bastian, who in this respect go much farther than Bastian himself. Others, while not denying the occurrence of historical connections, regard them as insignificant in results and in theoretical importance as compared to the working of the uniform laws governing the human mind. This is the view of by far the greater number of living anthropologists.

This modern view is founded on the observation that the same ethnical phenomena occur among the most diverse peoples, or, as Bastian says, on the appalling monotony of the fundamental ideas of mankind all over the globe. The metaphysical notions of man may be reduced to a few types which are of universal distribution; the same is the case in regard to the forms of society, laws and inventions. Furthermore, the most intricate and apparently illogical ideas and the most curious and complex customs appear among a few tribes here and there in such a manner that the assumption of a common historical origin is excluded. When studying the culture of any one tribe, more or less close analoga of single traits of such a culture may be found among a great diversity of peoples. Instances of such analoga have been collected to a vast extent by Tylor, Spencer, Bastian, Andree, Post and many others, so that it is not necessary to give here any detailed proof of this fact. The idea of a future life, the one underlying shamanism; inventions such as fire and the bow; certain elementary features of grammatical structure—these will suggest the classes of phenomena to which I refer. It follows from these observations that when we find an analogon of single traits of culture among distant peoples, the presumption is not that there has been a common historical source, but that they have arisen independently.

* Paper read at the meeting of the A. A. A. S. at Buffalo.

But the discovery of these universal ideas is only the beginning of the work of the anthropologist. Scientific inquiry must answer two questions in regard to them: First, what is their origin? and second, how do they assert themselves in various cultures?

The second question is the easier one to answer. The ideas do not exist everywhere in identical form, but they vary. Sufficient material has been accumulated to show that the causes of these variations are either external, that is founded in environment—taking the term environment in its widest sense—or internal, that is founded on psychological conditions. The influence of external and internal factors upon elementary ideas embodies one group of laws governing the growth of culture. Therefore, our endeavors must be directed to showing how such factors modify elementary ideas.

The first method that suggests itself and which has been generally adopted by modern anthropologists is to isolate and classify causes by grouping the variants of certain ethnological phenomena according to external conditions under which the people live, among whom they are found, or to internal causes which influence their minds; or inversely, by grouping these variants according to their similarities. Then the correlated conditions of life may be found.

By this method we begin to recognize even now with imperfect knowledge of the facts what causes may have been at work in shaping the culture of mankind. Friedrich Ratzel and W J McGee have investigated the influence of geographical environment on a broader basis of facts than Ritter and Guyot were able to do at their time. Sociologists have made important studies on the effects of the density of population and of other simple social causes. Thus the influence of external factors upon the growth of society is becoming clearer.

The effects of psychical factors are also being studied in the same manner. Stoll has tried to isolate the phenomena of suggestion and of hypnotism and to study the effects of their presence in the cultures of various peoples. Inquiries into the mutual relations of tribes and peoples begin to show that certain cultural elements are easily assimilated while others are rejected, and the time-worn phrases of the imposition of culture by a more highly civilized people upon one of lower culture that has been conquered are giving way to more thorough views on the subject of exchange of cultural achievements. In all these investigations we are using sound, inductive methods in order to isolate the causes of observed phenomena.

The other question in regard to the universal ideas, namely that of their origin, is much more difficult to treat. Many attempts have been made to discover the causes which have led to the formation of ideas 'that develop with iron necessity wherever man lives.' This is the most difficult problem of anthropology and we may expect that it will baffle our attempts for a long time to come. Bastian denies that it is possible to discover the ultimate sources of inventions, ideas, customs and beliefs which are of universal occurrence. They may be indigenous, they may be imported, they may have arisen from a variety of sources, but they are there. The human mind is so formed that it invents them spontaneously or accepts them whenever they are offered to it. This is the much misunderstood elementary idea of Bastian.

To a certain extent the clear enunciation of the elementary idea gives us the psychological reason for its existence. To exemplify: the fact that the land of the shadows is so often placed in the west suggests the endeavor to localize it at the place where the sun and the stars vanish. The mere statement that primitive man considers animals as gifted with all the qualities of

man shows that the analogy between many of the qualities of animals and human qualities has led to the generalization that all the qualities of animals are human. In other cases the causes are not so self-evident. Thus the question why all languages distinguish between the self, the person addressed and the person spoken of, and why most languages do not carry out this sharp, logical distinction in the plural is difficult to answer. The principle when carried out consistently requires that in the plural there should be a distinction between the 'we' expressing the self and the person addressed and the 'we' expressing the self and the person spoken of, which distinction is found in comparatively few languages only. The lesser liability to misunderstandings in the plural explains this phenomenon partly but hardly adequately. Still more obscure is the psychological basis in other cases, for instance, in that of widely spread marriage customs. Proof of the difficulty of this problem is the multitude of hypotheses that have been invented to explain it in all its varied phases.

In treating this, the most difficult problem of anthropology, the point of view is taken that if an ethnological phenomenon has developed independently in a number of places its development has been the same everywhere; or, expressed in a different form, that the same ethnological phenomena are always due to the same causes. This leads to the still wider generalization that the sameness of ethnological phenomena found in diverse regions is proof that the human mind obeys the same laws everywhere. It is obvious that if different historical developments could lead to the same results, that then this generalization would not be tenable. Their existence would present to us an entirely different problem, namely, how it is that the developments of culture so often lead to the same results. It must, therefore, be clearly understood that anthropological research which compares similar cultural phenomena from various parts of the world, in order to discover the uniform history of their development, makes the assumption that the same ethnological phenomenon has everywhere developed in the same manner. Here lies the flaw in the argument of the new method, for no such proof can be given. Even the most cursory review shows that the same phenomena may develop in a multitude of ways.

I will give a few examples: Primitive tribes are almost universally divided into clans which have totems. There can be no doubt that this form of social organization has arisen independently over and over again. The conclusion is certainly justified that the psychical conditions of man favor the existence of a totemistic organization of society, but it does not follow that totemistic society has developed everywhere in the same manner. Dr. Washington Matthews has shown that the totems of the Navajo have arisen by association of independent clans. Capt. Bourke has pointed out that similar occurrences gave origin to the Apache clans, and Dr. Fewkes has reached the same conclusion in regard to some of the Pueblo tribes. On the other hand, we have proof that clans may originate by division. I have shown that such events took place among the Indians of the North Pacific coast. Association of small tribes, on the one hand, and disintegration of increasing tribes, on the other, has led to results which appear identical to all intents and purposes.

Here is another example. Recent investigations have shown that geometrical designs in primitive art have originated either from naturalistic forms which were gradually conventionalized or from technical motives, or that they were primarily geometrical or that they were derived from symbols. From all these sources the same

forms have developed. Out of designs representing diverse objects grew in course of time frets, meanders, crosses and the like. Therefore the frequent occurrence of these forms proves neither common origin nor that they have always developed according to the same psychical laws. On the contrary, the identical result may have been reached on four different lines of development and from an infinite number of starting points.

Another example may not be amiss: The use of masks is found among a great number of peoples. The origin of the custom of wearing masks is by no means clear in all cases, but a few typical forms of their use may easily be distinguished. They are used for deceiving spirits as to the identity of the wearer. The spirit of a disease who intends to attack the person does not recognize him when he wears a mask, and the mask serves in this manner as a protection. In other cases the mask represents a spirit which is personified by the wearer, who in this shape frightens away other hostile spirits. Still other masks are commemorative. The wearer personifies a deceased person whose memory is to be recalled. Masks are also used in theatrical performances illustrating mythological incidents.*

These few data suffice to show that the same ethnical phenomenon may develop from different sources. The simpler the observed fact, the more likely it is that it may have developed from one source here, from another there.

Thus we recognize that the fundamental assumption which is so often made by modern anthropologists cannot be accepted as true in all cases. We cannot say that the occurrence of the same phenomenon is always due to the same causes, and that thus it is proved that the human mind obeys the same laws everywhere. We must demand that the causes from which it developed be investigated and that comparisons be restricted to those phenomena which have been proved to be effects of the same causes. We must insist that this investigation be made a preliminary to all extended comparative studies. In researches on tribal societies those which have developed through association must be treated separately from those that have developed through disintegration. Geometrical designs which have arisen from conventionalized representations of natural objects must be treated separately from those that have arisen from technical motives. In short, before extended comparisons are made, the comparability of the material must be proved.

The comparative studies of which I am speaking here attempt to explain customs and ideas of remarkable similarity which are found here and there. But they pursue also the more ambitious scheme of discovering the laws and the history of the evolution of human society. The fact that many fundamental features of culture are universal, or at least occur in many isolated places, interpreted by the assumption that the same features must always have developed from the same causes, leads to the conclusion that there is one grand system according to which mankind has developed everywhere; that all the occurring variations are no more than minor details in this grand uniform evolution. It is clear that this theory has for its logical basis the assumption that the same phenomena are always due to the same causes. To give an instance: We find many types of structure of family. It can be proved that paternal families have often developed from maternal ones. Therefore, it is said, all paternal families have developed from maternal ones. If we do not make the assumption that the same phenomena have everywhere developed from the same causes, then we may

* See Richard Andree. Ethnographische Parallelen und Vergleiche. Neue Folge, 1889, pp. 107 ff.

just as well conclude that paternal families have in some cases arisen from maternal institutions, in other cases in other ways. To give another example: Many conceptions of the future life have evidently developed from dreams and hallucinations. Consequently, it is said, all notions of this character have had the same origin. This is also true only if no other causes could possibly lead to the same ideas.

We have seen that the facts do not favor the assumption of which we are speaking at all; that they much rather point in the opposite direction. Therefore we must also consider all the ingenious attempts at constructions of a grand system of the evolution of society as of very doubtful value, unless at the same time proof is given that the same phenomena could not develop by any other method. Until this is done, the presumption is always in favor of a variety of courses which historical growth may have taken.

It will be well to restate at this place one of the principal aims of anthropological research. We agreed that certain laws exist which govern the growth of human culture, and it is our endeavor to discover these laws. The object of our investigation is to find the *processes* by which certain stages of culture have developed. The customs and beliefs themselves are not the ultimate objects of research. We desire to learn the reasons why such customs and beliefs exist—in other words, we wish to discover the history of their development. The method which is at present most frequently applied in investigations of this character compares the variations under which the customs or beliefs occur and endeavors to find the common psychological cause that underlies all of them. I have stated that this method is open to a very fundamental objection.

We have another method, which in many respects is much safer. A detailed study of customs in their bearings to the total culture of the tribe practicing them, and in connection with an investigation of their geographical distribution among neighboring tribes, afford us almost always a means of determining with considerable accuracy the historical causes that led to the formation of the customs in question and to the psychological processes that were at work in their development. The results of inquiries conducted by this method may be three-fold. They may reveal the environmental conditions which have created or modified cultural elements; they may clear up psychological factors which are at work in shaping the culture; or they may bring before our eyes the effects that historical connections have had upon the growth of the culture.

We have in this method a means of reconstructing the history of the growth of ideas with much greater accuracy than the generalizations of the comparative method will permit. The latter must always proceed from a hypothetical mode of development, the probability of which may be weighed more or less accurately by means of observed data. But so far I have not yet seen any extended attempt to prove the correctness of a theory by testing it at the hand of developments with whose histories we are familiar. This method of starting with a hypothesis is infinitely inferior to the one in which by truly inductive processes the actual history of definite phenomena is derived. The latter is no other than the much ridiculed historical method. Its way of proceeding is, of course, no longer that of former times when slight similarities of culture were considered proofs of relationships, but it duly recognizes the results obtained by comparative studies. Its application is based, first of all, on a well-defined, small geographical territory, and its comparisons are not extended beyond the limits of the cultural

area that forms the basis of the study. Only when definite results have been obtained in regard to this area is it permissible to extend the horizon beyond its limits, but the greatest care must be taken not to proceed too hastily in this, as else the fundamental proposition which I formulated before might be overlooked, viz: that when we find an analogy of single traits of culture among distant peoples the presumption is not that there has been a common historical source, but that they have arisen independently. Therefore the investigation must always demand continuity of distribution as one of the essential conditions for proving historical connection, and the assumption of lost connecting links must be applied most sparingly. This clear distinction between the new and the old historical methods is still often overlooked by the passionate defenders of the comparative method. They do not appreciate the difference between the indiscriminate use of similarities of culture for proving historical connection and the careful and slow detailed study of local phenomena. We no longer believe that the slight similarities between the cultures of Central America and of eastern Asia are sufficient and satisfactory proof of a historical connection. On the contrary, analogy of other similarities make such a connection improbable. But, on the other hand, no unbiased observer will deny that there are very strong reasons for believing that a limited number of cultural elements found in Alaska and in Siberia have a common origin. The similarities of inventions, customs and beliefs, together with the continuity of their distribution through a comparatively small area, are a satisfactory proof of this opinion. But it is not possible to extend this area safely beyond the limits of Columbia River in America and northern Japan in Asia. This method of anthropological research is represented in our country by Prof. F. W. Putnam and Prof. Otis T. Mason; in England by Dr. E. B. Tylor; in Germany by Friedrich Ratzel and his followers.

It seems necessary to say a word here in regard to an objection to my arguments that will be raised by investigators who claim that similarity of geographical environment is a sufficient cause for similarity of culture, that is to say, that, for instance, the geographical conditions of the plains of the Mississippi basin necessitate the development of a certain culture. There are those who would even go so far as to believe that similarity of form of language may be due to environmental causes. Environment has a certain limited effect upon the culture of man, but I do not see how the view that it is the primary moulder of culture can be supported by any facts. A hasty review of the tribes and peoples of our globe shows that people most diverse in culture and language live under the same geographical conditions, as proof of which may be mentioned the ethnography of East Africa or of New Guinea. In both these regions we find a great diversity of customs in small areas. But much more important is this: Not one observed fact can be brought forward in support of this hypothesis which cannot be much better explained by the well known facts of diffusion of culture; for archæology as well as ethnography teach us that intercourse between neighboring tribes has always existed and has extended over enormous areas. In the Old World the products of the Baltic found their way to the Mediterranean and the works of art of the eastern Mediterranean reached Sweden. In America the shells of the ocean found their way into the innermost parts of the continent and the obsidians of the West were carried to Ohio. Intermarriages, war, slavery, trade, have been so many sources of constant introduction of foreign cultural elements, so that an assimilation of culture must have taken

place over continuous areas. Therefore, it seems to my mind that where among neighboring tribes an immediate influence of environment cannot be shown to exist, the presumption must always be in favor of historical connection. There has been a time of isolation during which the principal traits of diverse cultures developed according to the character and environment of the tribes. But the stages of culture representing this period have been covered with so much that is new and that is due to contact with foreign tribes that they cannot be discovered without the most painstaking isolation of foreign elements.

The immediate results of the historical method are, therefore, histories of the cultures of diverse tribes which have been the subject of study. I fully agree with those anthropologists who claim that this is not the ultimate aim of our science, because the general laws, although implied in such a description, cannot be clearly formulated nor their relative value appreciated without a thorough comparison of the manner in which they assert themselves in different cultures. But I insist that the application of this method is the indispensable condition of sound progress. The psychological problem is contained in the results of the historical inquiry. When we have cleared up the history of a single culture and understand the effects of environment and the psychological conditions that are reflected in it we have made a step forward, as we can then investigate in how far the same causes or other causes were at work in the development of other cultures. Thus by comparing histories of growth general laws may be found. This method is much safer than the comparative method, as it is usually practiced, because instead of a hypothesis on the mode of development actual history forms the basis of our deductions.

The historical inquiry must be considered the critical test that science must require before admitting facts as evidence. By its means the comparability of the collected material must be tested, and uniformity of processes must be demanded as proof of comparability. It may also be mentioned that when historical connection between two phenomena can be proved, they must not be admitted as independent evidence.

In a few cases the immediate results of this method are of so wide a scope that they rank with the best results that can be attained by comparative studies. Some phenomena have so immense a distribution that the discovery of their occurrence over very large continuous areas proves at once that certain phases of the culture in these areas have sprung from one source. Thus are illuminated vast portions of the early history of mankind. When Prof. Morse showed that certain methods of arrow release are peculiar to whole continents it became clear at once that the common practice that is found over a vast area must have had a common origin. When the Polynesians employ a method of fire making consisting in rubbing a stick along a groove, while almost all other peoples use the fire drill, it shows their art of fire making has a single origin. When we notice that the ordeal is found all over Africa in certain peculiar forms, while in those parts of the inhabited world that are remote from Africa it is found not at all or in rudimentary forms only, it shows that the idea as practiced in Africa had one single origin.

The great and important function of the historical method of anthropology is thus seen to lie in its ability to discover the processes which in definite cases led to the development of certain customs. If anthropology desires to establish the laws governing the growth of culture it must not confine itself to comparing the results of the growth alone, but whenever such is feasible

it must compare the processes of growth, and these can be discovered by means of studies of the cultures of small geographical areas.

Thus we have seen that the comparative method can hope to reach the grand results for which it is striving only when it bases its investigations on the historical results of researches which are devoted to laying clear the complex relations of each individual culture. The comparative method and the historical method, if I may use these terms, have been struggling for supremacy for a long time, but we may hope that each will soon find its appropriate place and function. The historical method has reached a sounder basis by abandoning the misleading principle of assuming connections wherever similarities of culture were found. The comparative method, notwithstanding all that has been said and written in its praise, has been remarkably barren of definite results, and I believe it will not become fruitful until we renounce the vain endeavor to construct a uniform systematic history of the evolution of culture, and until we begin to make our comparisons on the broader and sounder basis which I ventured to outline. Up to this time we have too much reveled in more or less ingenious vagaries. The solid work is still all before us.

Franz Boas.

---

*PRINCETON IN THE NATION'S SERVICE.**

It used to be taken for granted—did it not?—that colleges would be found always on the conservative side in politics (except on the question of free trade); but in this latter day a great deal has taken place which goes far toward discrediting the presumption. The college in our day lies very near, indeed, to the affairs of the world. It is a place of the latest experiments; its laboratories are brisk with the spirit of discovery; its lecture rooms resound with the discussion of new theories of life and novel programmes of reform. There is no radical like your learned radical, bred in the schools; and thoughts of revolution have in our time been harbored in universities as naturally as they were once nourished among the Encyclopedists. It is the scientific spirit of the age which has wrought the change. I stand with my hat off at very mention of the great men who have made our age an age of knowledge. No man more heartily admires, more gladly welcomes, more approvingly reckons the gain and the enlightenment that have come to the world through the extraordinary advances in physical science which this great age has witnessed. He would be a barbarian and a lover of darkness who should grudge that great study any part of its triumph. But I am a student of society and should deem myself unworthy of the comradeship of great men of science should I not speak the plain truth with regard to what I see happening under my own eyes. I have no laboratory but the world of books and men in which I live; but I am much mistaken if the scientific spirit of the age is not doing us a great disservice, working in us a certain great degeneracy. Science has bred in us a spirit of experiment and a contempt for the past. It has made us credulous of quick improvement, hopeful of discovering panaceas, confident of success in every new thing.

I wish to be as explicit as carefully chosen words will enable me to be upon a matter so critical, so radical as this. I have no indictment against what science has done: I have only a warning to utter against the atmosphere which has stolen from laboratories into lecture rooms and into the general air of the world at large. Science—our science—is new. It is a child of the ninteenth century. It has trans-

* Concluding part of Prof. Woodrow Wilson's oration at the Princeton Sesquicentennial Exercises. Reprinted from *The Forum* for December, 1896.

formed the world and owes little debt of obligation to any past age. It has driven mystery out of the Universe; it has made malleable stuff of the hard world, and laid it out in its elements upon the table of every class-room. Its own masters have known its limitations; they have stopped short at the confines of the physical universe; they have declined to reckon with spirit or with the stuffs of the mind, have eschewed sense and confined themselves to sensation. But their work has been so stupendous that all other men of all other studies have been set staring at their methods, imitating their ways of thought, ogling their results. We look in our study of the classics nowadays more at the phenomena of language than at the movement of spirit; we suppose the world which is invisible to be unreal; we doubt the efficacy of feeling and exaggerate the efficacy of knowledge; we speak of society as an organism and believe that we can contrive for it a new environment which will change the very nature of its constituent parts; worst of all, we believe in the present and in the future more than in the past, and deem the newest theory of society the likeliest. This is the disservice scientific study has done us; it has given us agnosticism in the realm of philosophy, scientific anarchism in the field of politics. It has made the legislator confident that he can create, and the philosopher sure that God cannot. Past experience is discredited, and the laws of matter are supposed to apply to spirit and the make-up of society.

Let me say once more, this is not the fault of the scientist; he has done his work with an intelligence and success which cannot be too much admired. It is the work of the noxious, intoxicating gas which has somehow got into the lungs of the rest of us from out the crevices of his workshop—a gas, it would seem, which forms only in the outer air, and where men do not know the right use of their lungs. I should tremble to see social reform led by men who had breathed it; I should fear nothing better than utter destruction from a revolution conceived and led in the scientific spirit. Science has not changed the laws of social growth or betterment. Science has not changed the nature of society, has not made history a whit easier to understand, human nature a whit easier to reform. It has won for us a great liberty in the physical world, a liberty from superstitious fear and from disease, a freedom to use nature as a familiar servant; but it has not freed us from ourselves. It has not purged us of passion or disposed us to virtue. It has not made us less covetous or less ambitious or less self-indulgent. On the contrary, it may be suspected of having enhanced our passions, by making wealth so quick to come, so fickle to stay. It has wrought such instant, incredible improvement in all the physical setting of our life, that we have grown the more impatient of the unreformed condition of the part it has not touched or bettered, and we want to get at our spirits and reconstruct them in like radical fashion by like processes of experiment. We have broken with the past and have come into a a new world.

Can any one wonder, then, that I ask for the old drill, the old memory of times gone by, the old schooling in precedent and tradition, the old keeping of faith with the past, as a preparation for leadership in days of social change? We have not given science too big a place in our education, but we have made a perilous mistake in giving it too great a preponderance in method over every other branch of study. We must make the humanities human again; must recall what manner of men we are; must turn back once more to the region of practicable ideals.

Of course, when all is said, it is not learning, but the spirit of service, that will give

a college place in the public annals of the Nation. It is indispensable, it seems to me, if it is to do its right service, that the air of affairs should be admitted to all its class-rooms. I do not mean the air of party politics, but the air of the world's transactions, the consciousness of the solidarity of the race, the sense of the duty of man toward man, of the presence of men in every problem, of the significance of truth for guidance as well as for knowledge, of the potency of ideas, of the promise and the hope that shine in the face of all knowledge. There is laid upon us the compulsion of the National life. We dare not keep aloof and closet ourselves while a nation comes to its maturity. The days of glad expansion are gone; our life grows tense and difficult; our resource for the future lies in careful thought, providence and wise economy; and and the school must be of the Nation.

I have had sight of the perfect place of learning in my thought, a free place and a various, where no man could be and not know with how great a destiny knowledge had come into the world—itself a little world; but not perplexed, living with a singleness of aim not known without; the home of sagacious men, hard-headed and with a will to know, debaters of the world's questions every day and used to the rough ways of democracy; and yet a place removed—calm Science seated there, recluse, ascetic, like a nun, not knowing that the world passes, not caring, if the truth but come in answer to her prayer; and Literature, walking within her open doors, in quiet chambers, with men of olden time, storied walls about her, and calm voices infinitely sweet; here 'magic casements, opening on the foam of perilous seas, in fairy lands forlorn,' to which you may withdraw and use your youth for pleasure; there windows open straight upon the street, where many stand and talk, intent upon the world of men and business. A place where ideals are kept in heart in an air they can breathe; but no fool's paradise. A place where to hear the truth about the past and hold debate about the affairs of the present, with knowledge and without passion; like the world in having all men's life at heart, a place for men and all that concerns them; but unlike the world in its self-possession, its thorough way of talk, its care to know more than the moment brings to light; slow to take excitement, its air pure and wholesome with a breath of faith; every eye within it bright in the clear day and quick to look toward heaven for the confirmation of its hope. Who shall show us the way to this place?

CURRENT NOTES ON PHYSIOGRAPHY.

RECENT UNITED STATES GEOLOGIC FOLIOS.

Recent folios of the Geologic Atlas of the United States contain more examples of physiographic features, well illustrated, described and explained, than can be here noted. The McMinnville, Tenn., folio reveals details of form and structure in a district that has been heretofore practically untouched since Safford's excellent description in the State Survey report many years ago. The Highland, at an elevation of about 1,000 feet, is surmounted on the east by the Cumberland plateau, 2,000 feet elevation, with outliers and deep marginal valleys, and broken on the northwest by the ragged rim that descends to the central basin. The Three Forks, Montana, folio includes the Madison Valley, a typical example of an extinct lake basin, forty miles long by ten wide, formed by warping a pre-existent mountain region, and drained by a thousand-foot gorge cut through the enclosing ridge. The lake sediments thus laid bare are about 1,000 feet thick and include thin layers of gray volcanic dust that fell into the lake, covered by thicker layers of reddish weathered dust that washed in from

the surrounding country; all these layers are now trenched by the axial and centripetal streams. The outlet gorge is so deep and narrow that access to the bench land of the lake basin is gained at present only by roads that cross over the enclosing ridge. This relation of terraced lake beds and outlet gorge is of frequent occurrence on small and large scale in the Montana Rockies, and has its homologue in the Vale of Kashmir, the control of human occupation and movement being much alike in all. The other folios are equally deserving of physiographic note.

TIDES OF THE BAY OF FUNDY.

BAYA FONDA, named so by the early Portuguese explorers because it reached far into the land, and now anglicized as the Bay of Fundy, has a world-wide reputation for its excessive tides, which by tradition reach seventy feet of rise and fall, and advance with the speed of a galloping horse, as many of us have learned at school. The facts as reported by Chalmers (Geol. Surv Canada, for 1894 (1895), rep't M.) are somewhat more sober, but extraordinary enough. From the mouth of the bay, 48 miles wide and 70 to 110 fathoms deep, the bottom rises at the rate of four feet a mile over a distance of about 145 miles to the head. On the coasts adjacent to the mouth, the spring tides vary from 12 to 18 feet. Within the bay the spring and neap tides are as follows: Digby neck, 22, 18; St. John, 27, 23; Petitcodiac river, 46, 36; Cumberland basin, 44, 35; Noel river in Cobequid bay, 53, 31; the last named being the greatest tidal oscillation in any part of the bay. The flood tide rises about 20 feet above mean sea level; the ebb falls the same amount below, leaving the branch bays empty or nearly so. The tidal bore is seen in Maccan river, entering Cumberland basin, but is stronger in Petitcodiac river, entering Shepody bay. At the bend of this river, by Moncton, 20 miles from the bay head, the bore is seen to best advantage; it rushes in 'as a foaming breaker, five or six feet high, with a velocity of five or six miles an hour.' The spring and neap tides here have 45 and 38 feet range. The ebb tide runs like a mill race; the water rapidly sinking, the bare muddy channel is exposed, and the river is reduced to a small meandering stream. It so remains about two hours, when the rushing waters of the bore are heard again, and the river is soon filled with their sweeping flood.

In this connection, reference may be made to an account of the bore from English sources, at Hang Chow, south of Shanghai, China, in the Annalen der Hydrographie for October.

HÖLZEL'S GEOGRAPHISCHE CHARAKTERBILDER.

THE 37th and latest number of this beautiful and unrivalled series of chromolithographs 'for school and house' (Hölzel, Vienna) represents the gorge of the Rhine, looking southward past the Lorelei, and displaying the gently rolling uplands, with quiet farming villages and broad wheat fields, seldom entered by the stranger, in emphatic contrast with the deep, steep-sided gorge, an artery throbbing with international life, occasionally holding a town of close-packed houses where the little delta of a side stream affords foothold. The descriptive text is prepared by Penck's competent hand, and describes three stages in the evolution of the region: The general denudation of an ancient mountain range, indicated by the bevelling of the extremely deformed strata, reducing them to a *Gebirgsrumpf*, a peneplain; a slight elevation, followed by the excavation of a shallow valley trough in the rolling peneplane, 60 to 80 m. deep and one or two miles wide, still floored with river gravel and alluvium (loess); and a much more recent elevation,

accompanied by the trenching of the gorge, about 150 m. beneath the floor of the trough. The cliff of the Lorelei and the narrows and rapids in the river beneath it are caused by a heavy quartzite bed.

THE WANDERING OF LAKE NOR.

Sven Hedin gives an interesting account of the apparent recent wandering of Lop (lake) Nor on the level floor of the Gobi desert in Central Asia (Peterm. Mitt., XLII., 1896, 201–205, maps), confirming the views of Richthofen as against those of Prshevalski. The desert basin contains a great accumulation of waste from the surrounding mountains; coarser and steeper sloping around the margin, finer and dead level in the central depression (790 m.); here the aneroid observed from place to place varies only with the weather and season. The chief river is the Yarkand, coming from the west, and at high water in the late summer bringing much silt; this tends to drive the lake to the southeast. The winters are prevailingly calm; but in summer the wind is generally from the northeast, often stormy, drifting the surface sand and bearing so much dust as to darken the sky (hence called Kara-buran, or black storm); this drives the lake to the southwest. The resultant of river and wind action is a southward migration of the lake, but from a comparison of various records, Hedin concludes that there is an intermittent shifting back and forth, according as this or that part of the plain is aggraded. Further account of Hedin's perilous journey across the region of dunes is given in the London Geographical Journal of October.

W. M. Davis.

Harvard University.

CURRENT NOTES ON ANTHROPOLOGY.

THE INDIANS OF THE NORTHWEST COAST.

In some recent publications Dr. Franz Boas has added to his valuable contributions to our knowledge of the tribes of the northwest coast.

The eleventh report of the British Association Committee is altogether from his researches. It gives notes on the shamans, and a description of their beliefs and customs; also a study of Tshimshian houses, the growth of children and linguistic particulars.

In the 'Internationales Archiv' (Bd. IX.) he reports a number of songs from the Kwakiutl Indians, giving the music, the original words and both a literal and a free translation.

From him also there appears an article on 'The Indians of British Columbia' in the Bulletin of the American Geographical Society (No. 3, 1896). This describes their general appearance, their relationship and the details of their ordinary life.

All this material is at first hand, drawn from his personal studies in this field of ethnography.

THE TEMPLE OF TEPOZTLAN.

The 'Bulletin' of the American Museum of Natural History for November 13th contains a short article by Mr. M. H. Saville on a ruined temple near Cuernavaca, Mexico. His description is accompanied by several plates and introduces a remarkable relic, hitherto unknown. He states that it is 'the only aboriginal structure still standing in Mexico to which we can probably assign a positive date.' This he thinks can be done by the hieroglyphic inscriptions on its stones, which read the year 10 Tochtli, 1502 of our era, in the reign of Ahuizotl. He acknowledges that the ruins look much older, but he does not mention the possibility that Ahuizotl may have merely inserted his tablet in a wall constructed long before. Both descriptions and plates are interesting.

ANCIENT AND MODERN UMBRIAN SKULLS.

In the 'Atti' of the Roman Society of

Anthropology (Vol. IV.), Prof. L. Moschen discusses the craniology of the Umbrians. Previous writers (Nicolucci, Calori, etc.) have asserted that the ancient Umbrians were slightly dolicocephalous or mixed. Moschen claims that there are no undoubted skulls of pure blood Umbrians of antiquity, and therefore that these older studies are of little or no value. He undertakes to study the true modern type, following the general terminology of Sergi. They present a large variety of skull forms, with a prevailing tendency to mesocephaly and leptorhiny. He divides his series into eight principal varieties or forms, the ovoids and ellipsoids predominating.

It is difficult to draw any racial conclusions from this study.

D. C. BRINTON.
UNIVERSITY OF PENNSYLVANIA.

NOTES ON INORGANIC CHEMISTRY.

THE question of impurities in atmospheric air is one that has been investigated by many chemists and from many standpoints, and one upon which much light has been thrown in recent years. One of the most important phases of this subject cannot be said to have yet approached a solution. That air in crowded rooms, and especially exhaled air, is poisonous is well known. The 'black hole of Calcutta' is but an extreme case of what we have all many times experienced in poorly ventilated audience rooms. These evil effects were early attributed to an excess of carbon dioxid, which was certainly present. When it was shown that carbon dioxid of itself is not a poison, a certain mysterious 'effluvium' in exhaled air was conjured up, but its nature eluded investigation. The reduced proportion of oxygen has also been considered a cause. None of these explanations, however, can be considered satisfactory. That the unpleasant effects of crowded rooms could be due to nitrites in the air has also been suggested, but never sufficiently investigated to give a satisfactory answer. A contribution to this phase of the subject has been made by Mr. George Defren at the Massachusetts Institute of Technology, and published in the *Technological Quarterly* (9: 238). His work was mainly confined to the determination of the quantity of nitrous acid (or nitrites) in the excellently ventilated rooms of the Institute Laboratory. After a brief summary of the work of previous investigators, Mr. Defren gives an account of his own experiments.

The method first used was to expose pure water in porcelain dishes for from one to nineteen hours, and then estimate the nitrites absorbed by Griess' reagent. In every case nitrates were found. The quantity was dependent on the time the water was exposed, and on the nature of the work being carried on in the room. The burning of illuminating gas seemed to occasion the formation of nitrites; whether this is due solely to the incomplete oxidation of the nitrogenous constituents of the gas, as Mr. Defren believes, or to a direct union of the nitrogen and oxygen of the air, may be an open question. The second method used was quantitative and consisted in drawing air into large bottles, adding water and analyzing after twelve or more hours. The amount found was small on clear days (as low as .014 part nitrous anhydrid in 10,000 parts air), but was increased by the burning of gas and the presence of the people in the rooms. The largest amount found was .07 parts in 10,000. Exhaled air was tested by blowing through water, but no trace of nitrites was found, contrary to expectation. Mr. Defren suggests that nitrites present would, by the large volume of oxygen present, be oxidized to nitrites, or even decomposed to nitrogen. A test showed that even air containing nitrites, when drawn through water, gave no reaction. On the other hand, water in

which the hands were washed showed a strong test for nitrites. It was also shown that the air was very thoroughly washed from nitrites during a heavy shower.

Mr. Defren's results seem conclusive as to the presence of nitrites in the air; that nitrites have a powerful effect upon the human organism is well known; whether the quantity in the air may become large enough, from lack of ventilation, to cause the well known symptoms of oppression, is a point left for further investigation. At all events Mr. Defren's work is a valuable contribution to the problem. A single criticism might be offered, that no account appears to be taken of the fact that nitrous acid, when in solution, is rapidly oxidized by hydrogen peroxid to nitric acid, and similarly ammonia is oxidized to nitrous acid. Wurster's work on this point seems not to be noticed, though the article (Ber. 19: 3206) is mentioned in a foot note. This reaction might cause Mr. Defren's results to be too low for the amount of nitrous acid in the air, and would probably account for the apparent absence of nitrites in air exhaled through water.

Apropos of air analysis, Dr. T. L. Phipson communicates to the last *Chemical News* a unique method of removing the oxygen from the air. A graduated bell jar filled with air is placed over water. Suspended in the bell jar and not touching the water is a mushroom, *Agaricus atramentarius*. Exposed now to the sunlight, the mushroom rapidly and *quantitatively* absorbs the oxygen of the air, the carbon dioxid given off dissolving in the water, which rises in the bell jar one-fifth of the height. The mushroom itself dries up, becoming mummified in nitrogen. If now a green plant, as *Lysimachia nummularia*, be placed alongside the mushroom, the latter will recommence slowly to vegetate, living on the oxygen furnished by the green plant.

J. L. H.

SCIENTIFIC NOTES AND NEWS.

THE details of the approaching meeting of the American Society of Naturalists can now be announced. After the business meeting, on the afternoon of Tuesday, December 29th, the 'Inheritance of Acquired Characteristics' will be discussed by Prof. C. S. Minot, Prof. J. M. Macfarlane, Prof. E. D. Cope and Prof. Wm. James. In the evening, at the Fogg Museum of Art, Harvard University, Cambridge, Mr. Alexander Agassiz will give a lecture, the subject of which will be announced later, and will afterwards hold a reception at his residence. At noon on Wednesday there will be a lecture by Prof. E. B. Wilson, on 'Recent Developments of the Cell Theory,' which will be followed by a luncheon, given by the President and Fellows of Harvard College. In the afternoon Mr. Agassiz will meet the Society in the Museum of Comparative Zoology and describe the Museum. The annual dinner of the Affiliated Societies will take place in the evening at the hotel Brunswick, at which an address will be given by the President, Prof. W. B. Scott.

As we have already stated, the ninth annual meeting of the Geological Society of America will be held at Washington on December 29th, 30th and 31st. The President, Prof. Joseph Le Conte, has chosen as the subject of his address 'The Different Kinds of Earth-crust Movements and their Causes.' Papers have been entered to be read by J. S. Diller, N. H. Darton, Frank Leverett, J. F. Kemp, C. Willard Hayes, Marius R. Campbell, J. B. Woodworth, C. H. Hitchcock, Robert Bell, J. W. Spencer, Ralph S. Tarr, H. L. Fairchild, Angelo Heilprin, George H. Barton, F. B. Taylor, Harry Fielding Reid, Bailey Willis, Warren Upham, Charles R. Keyes, J. E. Wolff, A. H. Brooks, Joseph F. James, Henry S. Williams, David White, Henry B. Kümmel, William M. Clark, T. W. Stanton, F. H. Knowlton, E. H. Barbour, G. Perry Grimsley, George P. Merrill, Alfred C. Lane, Arthur Keith and G. K. Gilbert.

WE regard it as somewhat unfortunate that after meeting at the same place as the Naturalists last winter the Geologists should this year

meet in Washington, and the Folklore Society in New York. If the 'Jurassic Formation on the Atlantic Coast' is discussed should this be done in Boston or in Washington? If arrangements are made for a winter meeting of Anthropologists should it be done in New York or in Boston? We should like to suggest that in any event all the societies meet in New York two years hence. At that time the new buildings of Columbia University can be placed at the disposal of the societies and will offer much of interest. New York University will also be established in its fine buildings on its picturesque site. Other attractions of great scientific interest will be the Aquarium, the new wings and collections of the American Museum of Natural History, the Botanical Gardens and perhaps the Zoological Park.

THE New York Aquarium in Castle Garden was opened to the public on December 10th. We hope to give in an early number some account of the excellent arrangements carried out under the direction of Dr. Tarleton H. Bean.

A COMMITTEE of the New York Zoological Society met a committee of the Sinking Fund Commission on December 11th, and formally asked for the setting aside of 261 acres of land in Bronx Park for the Zoological Park. The members of the committee said that the Society would raise $250,000 for the purpose, and that they would collect $100,000 of this sum as soon as the necessary land had been set aside for the Park. The Mayor and Sinking Fund Commission will, we trust, decide in favor of a plan so important for the city.

THE Derby and Mayer Museums, Liverpool, will be enlarged at a cost of £72,000, but part of the building will be used for technical schools.

IT is now proposed that the Sedgwick Memorial Museum, Cambridge University, be erected upon a portion of the Downing College site, and that the grace of the Senate which assigned a site for the building on the old Botanic Garden be rescinded. If the site be changed it will be necessary to make new plans for the building.

THE Hebdomadal Council of Oxford University has received and accepted an offer from Professor Poulton to present a statue of Charles Darwin, to be made by Mr. Pinker, and to be placed in the court of the University Museum.

SIR JOSEPH PRESTWICH bequeathed, to take effect on the death of Lady Prestwich, £800 to the Geological Society of London for the establishment of a medal and fund to be awarded once in three years. He also left his collection of pamphlets and reprints to the Society.

THE Paris Academy of Sciences selected, on November 23d, two candidates for the directorship of the Observatory of Paris, vacant by the death of Tisserand. In the first ballot M. Loewy received 42 votes, M. Callandreau 18, and M. Poincaré 1. In the second ballot M. Callandreau received 48 votes, M. Stéphan 2, and MM. Poincaré and Wolf each 1. MM. Loewy and Callandreau are consequently the two candidates presented by the Academy to the Minister of Public Instruction. At the same meeting of the Academy M. Michel Lévy was elected a member of the section of mineralogy, in the place of the late M. Daubrée. He received 54 votes, 2 being given to M. de Lapparent.

PROF. ALPHEUS PACKARD was, on October 1st, elected an honorary member of the Russian Entomological Society.

DR. LEONARD J. SANFORD, who formerly occupied the chair of anatomy and physiology in the medical department of Yale University, died in New Haven on December 12th, at the age of 64 years.

DR. EMIL WOLFF, the chemist, died at Stuttgart on December 7th.

ACCORDING to *Garden and Forest* Mr. C. G. Pringle has just returned from another botanical journey in Mexico, where, during the past season, he has secured about 20,000 herbarium specimens in the valley of Mexico and in Cuernavaca.

DR. NANSEN has ordered a yacht of twenty tons to be built at Laurvig, and intends to continue on it his studies of the coast of Norway and Spitzberg.

THE following items of news are from the current number of *Natural Science*. A portion of the bequest made to the Swedish Academy of Sciences by A. F. Regnell, whose fortune was make as a physician in Brazil, was set aside by

the Academy to promote the study of the Brazilian flora. Every six years it yields about £1,150, which is applied in sending two Swedish botanists to Brazil for two years. The first expedition was undertaken by Drs. C. A. M. Lindman and G. O. A. Malme, who in 1892–94 explored Rio Grande, Paraguay and Matto Grosso. Dr. T. N. Tschernyschew, of the Russian Geological Survey, has completed his observations on Nova Zembla. This island bears traces like those in north Russia, of a formidable glaciation, followed by subsidence, during which the whole territory was transformed into an archipelago. Terraces, containing the shells of arctic molluscs, extend along the shores to a height of 160 meters. The present glaciers are in a period of growth. Prof. F. Regel, of Jena, went to Columbia last July on a voyage of exploration, to last some nine months. M. Maindron, the entomologist, has been commissioned by the French government to make a collecting expedition to the Persian Gulf and India.

The publication of *The Astronomical Journal* will be continued by Dr. S. C. Chandler, who cooperated with Dr. Gould in editing the *Journal*. With the assent of the family, it has been decided to place in the title-heading of each number, and upon the title-page of each volume, the words 'Founded by B. A. Gould,' as a permanent memorial.

The Torrey Botanical Club, New York, offers to supply a complete set of the *Memoirs*, five volumes, of which a considerable number remain undistributed, published at $15.00, for half price, or $7.50, the offer to hold good only until January 1, 1897. It is proposed to apply the money derived from these sales to reprinting the numbers of the *Bulletin* which are out of print.

The Commissioner of Labor, Mr. Wright, has submitted to the House of Representatives a plan for the organization of a permanent Census Bureau in Washington. He proposes a separate Census Bureau under the control of a Director of the Census, at a salary of $6,000 a year, with an assistant, at $4,000 a year. The other officers include a chief clerk, at $2,500; a disbursing clerk, at $2,500, and five chief statisticians, at $3,000, each, a year. Such further assistance as may be necessary shall be employed from time to time, the appointments being under civil service laws. It is provided that a general census shall be taken April 15, 1900, and every ten years thereafter, which will include a wide field of inquiry and investigation, of which we hope to give the details when settled.

We called attention last week to the meeting of the New York State Science Teachers' Association, at Syracuse, on December 29th, 30th and 31st. The leaders in the discussions have now accepted and are as follows: *Physics and Chemistry*, Prof. A. L. Arey, of the Rochester Free Academy, and Dr. E. L. Nichols, of Cornell University; *Physical Geography and Geology*, Prof. R. S. Tarr, of Cornell University; Dr. Richard E. Dodge, of the Teachers' College, New York City, and Dr. Frank McMurry, of the Buffalo School of Pedagogy; *Biology*, Prof. Charles Wright Dodge, of Rochester University, and Dr. Thomas B. Stowell, Principal of the Potsdam Normal School.

The anniversary meeting of the Royal Society was held at Burlington House on November 30th. The President, Sir Joseph Lister, made an address. He said, according to the report in the in the *British Medical Journal*, that the Council, in framing new rules for the conduct of the meetings and as to the publications of the Society, had desired, in the first place, to increase the interest of the meetings by giving greater freedom in their conduct and by enlarging the opportunities for discussion; and in the second place to obtain more secure and more rapid judgment on the value of papers, so that while the high standard of the *Philosophical Transactions* was retained, or even raised, greater rapidity in the publication both of them and of the *Proceedings* might be attained. The International Conference on the desirability and possibility of compiling and publishing, by international cooperation, a complete catalogue of scientific literature had agreed to the proposal, and had advised that the scheme should be carried out by an international central bureau, under the direction of an international council. Steps had been taken to encourage the practice

of appending subject indices to the papers published by the Society. Sir Joseph Lister then referred to the universal attention attracted to Röntgen's great discovery. He then passed in review some of the more important papers communicated to the Society during the year, and, in speaking of biology, alluded especially to the work of Prof. Schäfer, Prof. Farmer and Mr. Lloyd Williams. The presentation of medals then took place in the manner that we have already announced. In the evening Sir Joseph Lister presided at a dinner at which were present many distinguished guests and a large number of Fellows of the Society.

THE Appalachian Mountain Club, of Boston, has secured new and commodious rooms in the Tremont Building. They are centrally located and with a fine view, stretching from Big Blue to Prospect (the highest elevations near Boston), and including the Charles River basin and the State House. The library is now safely housed in a fire-proof building. The rooms are used for Council and Committee meetings, exhibitions and small social gatherings; they contain the library and collections, and are the workshop and office headquarters of the Club. The rooms are under the general supervision of the Room Committee and are open to all members from 3 to 5:30 p. m., under the charge of volunteer custodians.

Natural Science gives some account of the Institute of Jamaica, taken from its report for the year ending March, 1896. The museum has been removed from the lower floor of Date Tree Hall to adjoining new premises. These consist of two floors, the lower devoted to the collections of the Government Geological Survey, the Jamaica woods and most of the natural history collections; the upper floor is filled with the anthropological and the rock and mineral collections. By reason of these changes the library accommodation has been greatly increased, and the art department given room to develop. An attempt has been made to increase the public interest in the museum and natural history generally by the publication of museum notes in the Kingston newspapers. There has been an increase of 28 per cent. in the attendance of visitors to the museum.

UNIVERSITY AND EDUCATIONAL NEWS.

PROF. M. J. ELROD, of the Illinois Wesleyan University, has resigned to accept the chair of biology in the Montana State University.

THE following further appointments have been made at the Massachusetts Institute of Technology: Samuel C. Prescott, instructor in biology; Edward M. Bragg, assistant in mechanical engineering; Leonard H. Goodhue, assistant in analytic chemistry; Amadeus W. Grabau, assistant in geology; Minor S. Jameson, assistant in civil engineering; Albert J. Wells, assistant in mechanical drawing; Clarence W. Perley, assistant in biology.

M. POINCARÉ, now professor of mathematical physics and the theory of probabilities in the faculty of sciences of the University of Paris, has, at his request, been transferred to the chair of mathematical astronomy. M. Boussinesq, now professor of mechanics, takes the chair vacated by M. Poincaré.

THERE is now once more a University of Paris. The various faculties at Paris have been reorganized with a certain freedom from government control. The inauguration of the University has been celebrated in the new buildings of the Sorbonne with suitable ceremonies, which included an oration by M. Lavisse.

DISCUSSION AND CORRESPONDENCE.

X-RAYS.

TO THE EDITOR OF SCIENCE: The remark has been frequently made that the almost complete absence of specular reflection in the case of the X-rays may depend upon the want of sufficient polish of the reflecting surface. So far as I have noticed, no one has tried the experiment with mercury as the reflector. Very simple arrangements would admit of obtaining a perfectly plane surface, which, as to reflecting power, would leave nothing to be desired. A concave surface of mercury might be found to converge the rays. M. CAREY LEA.

REPLY TO PROFESSOR HALSTED.

IN the last number of SCIENCE Prof. Halsted, by bringing into juxtaposition two statements which were widely separated in a recent note

of mine published in SCIENCE, appears to convict me of inconsistency and to derive the conclusion that certain subjects introduced in an illogical manner into his 'Elements of Geometry' find a satisfactory treatment in his 'Elementary Synthetic Geometry.' These subjects are *the straight line as a minimum length* and the general notion of *the length of a curved line.* As a matter of fact, neither of these subjects is discussed in the latter work. The only curved line there considered is the circle.

THOMAS S. FISKE.

A QUESTION OF CLASSIFICATION.

TO THE EDITOR OF SCIENCE: The communications by Profs. Hollick and Ward in your recent numbers, commenting upon Prof. Marsh's determination of the Jurassic age of the Potomac and Amboy clays of the south New England island series and New Jersey-Virginia coast, and Prof. Marsh's reply to Prof. Hollick in the November number of the *American Journal of Science*, are of interest to all workers in American Mesozoic formations.

Prof. Marsh, in his early papers on the vertebrates of the Atlantosaurus beds of the age of Colorado and the Potomac beds, has referred both of these to the Wealden epoch. American geologists * do not assert the existence, in this country along the present Atlantic slope, of Jurassic beds of Atlantic sedimentation representing the whole or part of that vast period of time below the Wealden. In view of these facts, it is no inference to state that the broad generic term 'Jurassic period,' as applied by Prof. Marsh to this portion of our country, is a synonym for the term Wealden epoch, as used by others. Hence that part of the controversy, so far as it involves the oldest or Potomac beds, narrows down to the question of whether the beds of the Wealden epoch should be classified as the top of the Jurassic or the base of the Cretaceous period of geologic time.

* With the exception of Prof. Jules Marcou, who originally maintained that the Middle and Lower Cretaceous of Texas and the Plains Tertiary were Jurassic, and who still maintains the Jurassic age of the Middle Cretaceous beds of New Mexico and the Lower Cretaceous of Texas. This position has been disproved by research.

This is an old and much discussed question of English geology. It would be impossible here to give even brief reference to the extensive literature of the question. It is sufficient to say that, after the most thorough sifting of the evidence pro and con, European opinion and usage of to-day uphold the Cretaceous age of the Wealden beds. Against the opinions of a few who hold to the contrary, a volume could be filled with the data of eminent European authorities who maintain the Cretaceous age of the Wealden, including L. Agassiz, Lyell, Jukes, Prestwich, Zittel, Etheridge, Woodward, Pavlow, Fischer and others. Even as I write these lines the mail brings, fresh from the press of the official Geological Survey of Great Britain, an elaborate monograph of five volumes on the English Jurassic by H. B. Woodward, which excludes the Wealden from the Jurassic and places it at the base of the Cretaceous period. Furthermore, the consensus of opinion in all the reports of the meetings of the International Congress of Geologists places the Wealden as the base of the Cretaceous system.

Prof. Marsh, however, has assumed the position in several writings that the Wealden epoch belongs to the Jurassic period and not to the Cretaceous, and this opinion explains his use of the term Jurassic in this country. On the other hand, all the able authorities of this country, except Prof. Marsh, who have studied or reviewed the Potomac and allied formations of Wealden affinities have reached the final conclusion that they are of Cretaceous age. Among these may be mentioned Dana, Newberry, Ward, McGee, Hollick and others. The U. S. Geological Survey has also mapped these formations as Cretaceous upon its latest atlas sheets.

The controversy, in part, thus narrows down to the problem of the age of the beds of the Wealden epoch, and naturally arouses an inquiry as to what criteria can be depended upon to settle the limitations of the geologic periods. There are three plausible methods that suggest themselves: (1) precedents and usage; (2) correspondence of the rock of each period with great cycles of sedimentation, and (3) the presence of characteristic, distinguishing fossils.

The weight of precedent and usage, as shown by the eminent authorities cited, undoubtedly assigns the Wealden beds to the base of the Cretaceous. In view of this almost unanimous opinion of the ablest individual authorities, the established usage of the official surveys of Great Britain and the United States, and the expression of the representative International Congresses of Geologists, to the effect that the Wealden is the base of the Cretaceous system, it wi require more than the assertion of one savant, however eminent in research, to change the accepted geologic classification. At least some preconcerted action and international agreement on the part of the geologic societies should be had before one would be justified in using the broad term Jurassic for beds which at the utmost could only doubtfully be referred to it, and which, if so referred, would represent only a minute fraction of the great time period of the Jurassic.

Periods of geologic time usually correspond with one or more great oscillations of land and accompanying migration of the marine shore lines, producing cycles of sedimentation. These cycles are recorded by successive variations in the character of the sediments manifested: First, by estuarine and other unsorted marginal deposits, representing the beginning of the subsidence. In turn these are succeeded by more finely sorted and deeper-water or off-shore beds, as subsidence of bottom and landward migration of shore progresses. The Wealden in England and the Potomac in America most clearly represent the basement littorals of the Lower Cretaceous epochs of sedimentation, belonging by every physical affinity and gradation with the overlying beds.

Furthermore, the Potomac from New Jersey nearly to the Rio Grande is undoubtedly a marginal, land-derived formation, laid down at oceanic deposition level, and one which marks the initiation of the great cycle of Lower Cretaceous sedimentation, recording the encroachment of the Cretaceous sea upon the pre-existing Jurassic continent. In Texas these beds certainly lie unconformably alike upon Algonkian, Silurian, Carboniferous, Permian and alleged Triassic, and there is not a trace of pre-existing Jurassic sediments.

The third method, based upon the presence of characteristic fossils, is likewise valuable, but least trustworthy. The land and fresh-water animals, land plants and marine mollusks each present a widely varying standard, and leave room for differences of opinion upon the part of their respective students. While each of these (except the fresh-water mollusks, which seem of little diagnostic value) has peculiar characteristics for each of the great periods, there is no reason to presume that research may not often lead to the discovery of the persistence of supposed characteristic Jurassic forms into the Cretaceous, or Cretaceous forms into the Tertiary, as has been done in some instances. In such cases, however, no one has ever changed the period designation of the beds. For instance, ammonites were once reported to be found in the Eocene of India, but no one has transposed the Eocene epoch from the Tertiary to the Cretaceous on that account, and even if Prof. Marsh has found Jurassic land vertebrates in the Wealden it is doubtful if he would be justified, in face of the opposing evidence of the plants, mollusks and sediments, in making such a radical step as transferring the Wealden beds from the Cretaceous to the Jurassic period. It would be far more logical, in my opinion, to consider that the vertebrate life of the Jurassic land has persisted slightly into the Cretaceous period.

Regardless of European analogy, however, there is every stratigraphic and paleontologic reason for placing the Potomac-Trinity formations of America as the base of the Cretaceous. In Texas, plants, vertebrates and marine mollusks are found associated in the basement Trinity beds, the equivalent of the lowest Potomac. The plant life and molluscan life which have been most thoroughly studied show no more Jurassic characters than are usually found in these basement beds.

With all due deference to the opinion of others, there are reasons for suspecting that no marine Jurassic formations of Atlantic sedimentation have as yet been discovered north of Argentina on the present Atlantic slope of the American hemisphere, and furthermore I hold that there are strong reasons for believing that this absence is due to the fact that the conti-

nental expansion towards the east was far greater in Jurassic time than now, especially in the tropical and South American regions. That lacustral deposits of the alleged nature of Prof. Marsh's Atlantosaurus beds of Wyoming and Colorado may have been made upon this Jurassic land mass is not only possible but plausible, but Jurassic deposits at marine sedimentation level are undoubtedly missing or undiscovered upon the Atlantic slopes of both American continents.

ROBT. T. HILL.
U. S. GEOLOGICAL SURVEY.

SINCE the above was written, Prof. Marsh has published another contribution upon 'The Jurassic Formation of the Atlantic Coast.'* This contains many statements with which American geologists will differ, and conflicts more or less directly the results of others, who for years have carefully explored and described the Mesozoic formations of this country.

Prof. Marsh, in his previous papers, has transferred the Wealden epoch from the base of the Lower Cretaceous to the top of the Jurassic; and the tenor of the present article is to repudiate the Lower Cretaceous entirely, as is shown in the diagram of the 'Geologic Horizons of Vertebrate Fossils,' and in the many places where he makes the Dakota Formation the base of the Cretaceous. Upon the evidence of plants, vertebrates and mollusks, all other students place the Dakota Formation in the middle of our American Upper Cretaceous and at the base only of the upper of the two great series into which the Cretaceous of this country is divided. It is considered also as the time equivalent to the Middle Cretaceous of Europe. Between the Dakota and Jurassic time positions, both in this country and Europe, there are extensive series of sediments representing the great interval of the Lower Cretaceous time. Even Prof. Jules Marcou, who has here alone upheld the Jurassic age of the Lower Cretaceous formations in part, admits that there are extensive Lower Cretaceous beds below the Dakota. These Lower Cretaceous beds, to which the Potomac belongs, and not the Dakota, as alleged by Prof. Marsh, are the true base of the Cretaceous system in the Rocky Mountain region, at least in the Trans-Pecos, Texas and Mexican portions of the Rocky Mountains. Neither is it improbable that they are the true base of the Cretaceous in the Colorado region, if the Atlantosaurus beds are of the Lower Cretaceous series. The latter clearly occupy the stratigraphic position where the Lower Cretaceous beds ought to be, lying beneath the Dakota and above the last determinable Jurassic. For years students of American stratigraphy have desired to know the relationship between these Atlantosaurus beds of Colorado and the nearest allied Potomac-like formations of undoubted Atlantic sedimentation. Some have even suspected that they might ultimately prove to be a part of the great basement littoral of the Lower Cretaceous of the Texas and Potomac regions. In a previous paper I have suggested that there might be stratigraphic relationship between the Trinity-like sands at the base of the Tucumcari series of New Mexico and the Atlantosaurus beds of Colorado. An attempt to trace this connection, however, resulted in the conclusion that it would be impossible to prove it, owing to discontinuity of outcrop along the flanks of the Rocky Mountains in New Mexico. Prof. Marsh, however, now assures us that the Atlantosaurus beds are the western extension of the Potomac formation of the Atlantic coast, and hence those who still believe in the Lower Cretaceous position of the latter, would hereafter be justified by his correlation in mapping the Atlantosaurus beds as Cretaceous.

An impression is obtained from Prof. Marsh's writings that his deductions are based entirely upon the vertebrates, and that he has not fully considered the correlative testimony of other life forms the species of which occur in greater abundance and have a more world-wide distribution than the vertebrates. In discussing the relative merits of plants and vertebrates as stratigraphic criteria, he even states that the attempt to make out the age of formations by the use of fossil plants is too often labor lost. Whatever may heretofore have been the diagnostic value of paleobotany in stratigraphic determination, the recent detailed researches in this country have created for it a position that com-

* This JOURNAL, December 5, 1896, and *American Journal of Science*, December, 1896.

mands respect. On the other hand, the discovery and description of vertebrates in America has been accomplished by so little stratigraphic and comparative data that the exact value of these forms as aids in stratigraphic interpretation is uncertain. A single contribution to the stratigraphy of the vertebrate beds, or critical comparison of the forms with European affinities, such as Prof. Ward has recently given us of the plants of the Potomac formation,* would be a most welcome contribution to American geology. In any event, ridicule cannot overcome the fact that research has not as yet shown the existence in the Jurassic of dicotyledonous plants, such as the Cretaceous beds of the Atlantic coast contain.

The following statements in Prof. Marsh's paper are also of interest: "The invertebrates known from these strata are few in number, but some of the mollusks among them point to the Jurassic age, as Whitfield has shown." "There is now positive proof that the southern end of this series is Jurassic, and it is certainly a fair conclusion that the remainder is of the same age. The burden of proof will rest upon those who hold to the contrary." The writer has been studying the southern extension of the Potomac formation in Arkansas and Texas for many years, but is not aware of Prof. Marsh's having ever examined the beds at all. There the continued Potomac beds, as above stated, contain plants, vertebrates and mollusks in intimate association. The counsel of the ablest authorities and specialists has been sought in the interpretation of these invertebrate fossils. With the exception of Prof. Ferdinand Roemer,† and Prof. Heilprin, who maintained that the beds were Upper Cretaceous, and Prof. Jules Marcou, who still believes them Jurassic, the Lower Cretaceous position of the beds is maintained by all other later authorities who have studied the plants, vertebrates and invertebrates. It is true that invertebrates are few in number in the North Atlantic States, but in the Texas-Arkansas region, 'the southern end of this series,' which Prof. Marsh says is also Jurassic, over 300 species have been noted from the beds below the Dakota which, as a whole, clearly testify to the Lower Cretaceous positions of the beds. In describing the New Jersey forms to which Prof. Marsh refers, Prof. Whitfield, instead of referring them positively to the Jurassic, clearly says: "We get no help of sufficient value to establish the geologic horizon of the beds from these molluscan remains, and aside from the evidence furnished by the plant remains we must rely entirely upon their stratigraphic position."*

It is strange that Prof. Marsh, while discussing the invertebrates and paleobotany, makes no mention of the true Lower Cretaceous vertebrates, and omits them from his 'Horizons of Vertebrate Fossils.' It is true that the vertebrates are rare and have been less fully studied than the plants and invertebrates, but Prof. Cope has already described five species of fishes from the Southwestern beds, and has referred them all to the Cretaceous. Williston† has likewise published several vertebrates from the Lower Cretaceous of Kansas, including turtles, fishes, saurians and crocodiles.

Prof. Marsh's views of continuous sedimentation along the Atlantic coast through the various periods of geologic time are also peculiar. He says: "To place the strata in question in the Jurassic section of the Atlantic coast at once removes many difficulties that have hitherto perplexed students of the Mesozoic of this region. It completes the series, and shows in part at least what was done in deposition during that long interval between the end of Triassic and the beginning of Cretaceous time." It is not exactly clear how the geologic series of the Atlantic coast will be completed by restoring the Jurassic sediments, as he proposes to do, at the expense of the

*Some Analogies in the Lower Cretaceous of Europe and America, by Lester Frank Ward. Extract from the Sixteenth Annual Report of the U. S. Geological Survey, 1894–95, Part I., Director's Report and Papers of a Theoretic Nature, 1896.

†Prof. Marsh lays stress upon the fact that Prof. Roemer told him to look out for the Jurassic in America. It may interest him to know that the writer has a letter from Prof. Roemer, written just before his death, in which he insists upon the Upper Cretaceous age of the Lower Cretaceous beds which Prof. Marsh is now including in the Jurassic.

*Monograph IX., U. S. Geol. Survey, p. 23, 1885.

† Kansas University Quarterly, July, 1894.

Lower Cretaceous beds, which are left out of the geologic column. A great break in the sedimentary sequence would still exist between the Wealden and Dakota. In the light of the testimony of structure and paleontology, the current hypothesis that land conditions prevailed in Jurassic time makes a much more harmonious and acceptable geologic record.

Personally, while differing with Prof. Marsh, the writer feels grateful that he has reopened this question, for we believe it will result in a more thorough understanding and appreciation of the Lower Cretaceous epoch and its influence in the making of our continental history. In conclusion, however, we must confess our inability to see that Prof. Marsh has submitted sufficient proof to maintain his proposition or to upset the accepted results of the minute geologic research throughout the Atlantic Coastal Plain. To prove these beds Jurassic by moving the boundary between periods is not an altogether satisfactory method, nor in harmony with geologic usage. Neither will the testimony of a few vertebrates in beds abounding in Cretaceous-like plants and invertebrates be of sufficient weight to upset the accepted nomenclature, especially when the time position of these vertebrates in the European standard to which they are referred is unknown.

Inasmuch as the evidence contrary to Prof. Marsh's position has all been brought out in accepted scientific literature, and he, as yet, has presented no detailed evidence to maintain his unique position, it is difficult to appreciate his statement that the burden of proof 'belongs upon those who hold contrary opinions' to himself. It appears instead that he is submitting data which may be used to advantage by those who might believe in the Cretaceous age of the beds which he has so long called Jurassic.

R. T. H.

PROFFESSOR WILSON'S ADDRESS AT THE PRINCETON SESQUICENTENNIAL CELEBRATION.

The concluding part of Professor Woodrow Wilson's oration at the Princeton sesquicentennial celebration has been received with general applause by literary and religious journals. The occasion of its delivery made it more than an individual utterance, for the speaker and the hearers must have understood it to present a program for Princeton University. Men of science should, therefore, read Professor Wilson's words in order that they may know of the existence of a point of view which they may have thought obsolete.

Professor Wilson holds that the scientific spirit of the age is 'doing us a great disservice, working in us a certain great degeneracy,' that the limitations of science are known to its own masters, who 'have eschewed sense and confined themselves to sensation.' He is indeed prepared to acknowledge certain achievements of science, but for him 'the scientist' seems to be the man who invents the steam engine or the sewing machine. The practical applications of physical science have, it is true, reformed the world. They have answered with facts Professor Wilson's predecessor whose *a priori* arguments claimed that population must increase more rapidly than the means of subsistence. They have made possible a civilization in which each man may have not only physical well-being, but also time and means for thought and culture. But I believe that science has done more than this; it has not only given opportunity for education and culture; it also offers the best means of culture and the truest standpoint from which to view the world. Keats might see no beauty in the rainbow after its causes had been explained to him, and Professor Wilson may think Phœbus and his horses a nobler conception than those of modern astronomy. But the man of science does not find that the beauty of the world becomes less, as he learns more of its order.

Scepticism, pessimism and the like are much older than the present century; they do not result from scientific study, as Professor Wilson claims, but are rather literary products. It is not the student of science, but Professor Wilson, who 'cowers' 'in an age of change.' If, as Professor Wilson says, classical studies make a boy a gentleman, scientific studies may make him a man. The present writer does not undervalue classical studies, but finds the difficulty to be that in a college such as Princeton the work with grammar and dictionary is a somewhat trivial science and the student does not go on far enough to appreciate classical literature and art or to undertake the scientific study of the causes of the

development of civilization. But Professor Wilson holds that science should confine itself to counting the chemical elements and becomes a 'noxious, intoxicating gas' when its methods are applied to the study of the development of society.

Views such as Professor Wilson offers on the limitations and evil effects of science seem like a survival from the denominational college of fifty years ago, and I regard it as unfortunate that they should have been presented in an official address at the inauguration of Princeton University.

J. MCKEEN CATTELL.

SCIENTIFIC LITERATURE.

Monograph of the Bombycine Moths of America, North of Mexico, including their Transformations and Origin of the Larval Markings and Armature. Part I., Family 1, the Notodontidæ. By ALPHEUS S. PACKARD. Nat. Acad. of Sci., Vol. VII., First Memoir. 1895. Pp. 291, 4to, plates 49, many colored, and 10 maps.

"I am greatly pleased," writes Dr. A. Spuler, of Erlangen, "when I note how much, in these latter days, the study of entomology in America is pursued by true zoologists, and not by mere dilletants." Dr. Spuler and other exponents of scientific entomology will be convinced in this belief if American entomology maintains the standard set for it by Dr. Packard's latest important work, the first part of his monograph of the Bombycine moths of North America.

It is with the chapters of the book included in its first eighty pages that my brief criticism will chiefly have to do. These introductory chapters present a discussion of the present knowledge of the phylogeny of the Lepidoptera, a knowledge to which Dr. Packard has been a conspicuous contributor, and with the details of which he is thoroughly conversant.

Since there have been students of insects there has been classification of insects. There have been pre-Darwinian and post-Darwinian classifications. But not until very recent years has there been much of a revealed phylogeny of insects. However fully and unreservedly we have, for years now, accepted the theory of descent, we have been, speaking for the while only of entomologists, very slow to align our work with our beliefs. We have been content w th Linnæan classifications. We have been inconsistent. We have let phylogeny and ontogeny mean to us—if, haply, they had any meaning for us—problems for the 'general zoologists,' the German morphologists and embryologists. But if we are Darwinians our systematic entomology must take on the aspect of phyletic study, and drop its too long persistent Linnæan character.

Of late, fortunately, there has appeared an awakening among American entomologists, and some notable progress has been made toward an appreciative recognition of the demands made upon us by our beliefs. This welcome beginning o the phylogenetic study of insects is specially noticeable in the treatment of the Lepidoptera. The recent studies of Comstock and Dyar, of Chapman (England) and of Spuler and Walter (Germany), combined with his own, have enabled Dr. Packard to present in the preliminary chapters of this monograph a suggestive and reasonable discussion of the phylogeny of the moths and butterflies. It would be ill advised to attempt to refer here to the details of this discussion; many of these details are yet moot points, most of them, indeed. There is yet no consensus of authority to refer to on these questions. There are not enough men competently familiar with the matters at issue to form a consensus of authority, if one may so put it. It is a bold undertaking, perhaps, to attempt, as yet, to arrange phyletically the species of a family of insects; but it is a praiseworthy undertaking, because it is consistency. Dr. Packard is a Neo-Lamarckian. He believes that he finds much evidence for Neo-Lamarckism in the adaptational characters of the larvæ and pupæ. A Neo-Darwinian might affirm that the author has assumed the truth of Neo-Lamarckism and has explained the origin and development of these characters in accordance with his belief. There is an unsatisfying character about the treatment of the interpolated adaptive characters of the immature stages. The categorical distinguishing between the adaptational and the congenital characters seems arbitrary. But any questioning of the interpretations or dissent from the conclusions contained in these chapters on the phylogeny of the Lepidotera cannot lessen

our enthusiastic appreciation of the character of the discussion. There is much inspiration for the right sort of entomology to be got from reading these eighty and odd pages.

It is that part of the work as yet unreferred to which alone is indicated by the title of the book. Much of the inspiration gained from the perusal of the introductory chapters would be lost if the purely systematic part of the work were not treated consistently with the author's beliefs. But the treatment is consistent. The Bombycine family Notodontidæ, including, according to Dr. Packard's delimitation of the group, '21 genera and about 78 species,' found in America, north of Mexico, is the subject of a careful monograph. The biology of each species is given as fully as known, a special attention being paid to the details of larval markings and armature. It is hardly necessary to say that careful descriptions of immature stages "in the light of the recent very suggestive and stimulating work of Weismann, entitled 'Studies in the Theory of Descent,'" are not so common in monographs of Lepidopterous families that, met with, they should pass without comment. Such treatment is distinctly rare. The detailed descriptions of these immature stages are supplemented by a splendid series of colored plates of larval forms. There are also a series of plates of wing venation, some figures of the external anatomy of the heads of imagines and a number of maps showing the geographical distribution of the family.

The book is a valuable one for its point of view as well as for the actual matter of it. American entomology will be helped by it in reputation and in inspiration.

Vernon L. Kellogg.
Stanford University, Calif.

Determinative Mineralogy and Blowpipe Analysis. By George J. Brush. Revised and enlarged by Samuel L. Penfield. New York, John Wiley & Sons. 1896. Pp. 163 and 33 double pages of tables. $3.50.

Mineralogists, metallurgists and students in these branches of science, who have been using the former edition of this book, on account of the value of text and tables, will gladly welcome this revised edition.

In this new edition, the text has been thoroughly revised and for the most part new material has been substituted. The work has been greatly enlarged by the addition of a new chapter and by the expansion of the chapter on 'The Reactions of the Elements.'

In the introductory chapter, the author has very clearly and concisely explained and defined the commoner terms and names used in mineralogy and also those in chemistry, necessitated by the study of the chemical character of the minerals.

The second chapter has been devoted, (1) to a description of the blowpipe apparatus "which is necessary or convenient for making the simple tests for the identification of the elements and the determination of the minerals;" (2) to the reagents commonly employed in the study of minerals; (3) 'to the nature and use of flames.' There is here a very full and clear description of the character and use of the different flames, well illustrated with cuts and descriptions of experiments on the composition and use of the different parts of the flame.

In the chapter on the reactions of the elements, which occupies nearly one hundred pages of the text, the elements have been taken up alphabetically, for convenience of reference. In connection with each of the elements, a short description is given of their occurrence and rarity. The tests described, which include a great many new ones especially devised for this work, have all been carefully verified by the author, and are applicable for the elements in their many forms of combination. Many of the old tests have been simplified and improved, and details are given concerning methods of manipulation in making many of the tests, which will be found exceedingly useful. This part of the book will be greatly appreciated by mineralogists on account of the thorough and exhaustive work that has been done in bringing the text up to date. In connection with most of the elements, experiments are very carefully described, by which the tests can be very characteristically illustrated. This adds greatly to the value of the work as a text-book. To facilitate the use of the work as such and also as a book of reference, the descriptions of the rarer elements are given in fine print, as well

as the experiments and "conspicuous headlines and catchwords have been freely used."

Another part of the work that has been most conveniently and systematically arranged is the chapter on 'The Important Blowpipe and Chemical Reactions.' It consists of a tabulated arrangement of the reactions observed and is 'intended to be used especially for the interpretation of unknown reactions which are encountered in blowpipe analysis,' and it can be made to serve as a course in qualitative blowpipe analysis in examining unknown substances.

The chapter introductory to the tables as well as the tables are the same as in the former edition, but as stated by the author in the preface: "A complete revision of the tables for the determination of minerals will be made as soon as possible, and a short chapter on crystallography and the physical properties of minerals will be prepared."

One feature of the book that especially commends it to the mineralogist is that in the tests taken up, no one arbitrary method is employed, but the best ones, whatever their character, are described, thus making the work general and covering all the physical, chemical and blowpipe tests useful for the identification of the elements and minerals.

J. H. PRATT.

YALE UNIVERSITY.

SOCIETIES AND ACADEMIES.

ENTOMOLOGICAL SOCIETY OF WASHINGTON.

DECEMBER 3, 1896.

R. H. PETTIT, St. Anthony Park, Minn., and F. A. Sirrine, Jamaica, N. Y., were elected Corresponding Members.

Under the head of exhibition of specimens, Mr. J. D. Patten showed living examples of *Lasioderma serricorne*, and exhibited a cigar from which the beetles had emerged. Mr. Ashmead exhibited a small collection of micro-Hymenoptera, made by Mr. Townsend at San Rafael, Mexico. Mr. Howard exhibited specimens of two new Coccidæ allied to *Icerya*.

A paper by Mr. T. D. A. Cockerell was read which consisted of notes on the recently published No. 1, Volume IV., of the Proceedings of the Society.

In the discussion of this paper the fact was brought out by Messrs. Ashmead and Schwarz that *Eciton* and *Labidus* are unquestionably distinct and that the true female of *Eciton* has been found in North Carolina by the Rev. P. Jerome Schmidt, who has had a good drawing of it in his possession for at least two years.

Mr. Schwarz presented some notes on the 'Lerp Insects' (Psyllidæ) of Australia. After reviewing the literature on the subject he discussed the various forms of cases spun by these Psyllid larvæ on the leaves of Eucalyptus trees, illustrating his remarks with drawings and exhibition of specimens. For the more or less conical larval cases which on the surface are provided with longitudinal ribs Signoret's generic name, *Spondyliaspis*, has to be accepted and includes the *Psylla eucalypti*, described by Dobson. The remarkable structure of the first joint of hind tarsi already observed by Dobson as well as the structure of the hind tibiæ, the posterior apical edge of which is produced into a stout mucro, fully justify the erection of a new sub-family under the name Spondyliaspinæ for *P. eucalypti* and congeneric species. Another Psyllid, the larvæ of which weaves the beautiful shell-like structures described by Dobson as his third form of lerp, was made by Mr. Schwarz the type of a new genus and species under the name *Cardiaspis artifex*.

This paper gave rise to an animated discussion participated in by Messrs. Gill, Stiles, Howard and Schwarz, on the advisability or necessity of the adoption of generic and other names based upon excretions of or structures formed by insects or their larvæ and by other animals, the animals themselves being unknown. The general opinion seemed to be that where such a structure or secretion is an expression of morphological character it has sufficient taxonomic value to carry the name.

L. O. HOWARD,
Secretary.

ANTHROPOLOGICAL SOCIETY OF WASHINGTON.

THE 254th regular meeting of the Anthropological Society was held Tuesday evening, December 1, 1896.

The first paper, by Mr. Arthur Bibbins, Professor of Geology of the Women's College of

Baltimore, Maryland, entitled 'Some Paleontologic Folk-Lore of Maryland,' illustrated by a collection of fossil cycads of the Potomac formation.

The ideas of the residents of the rural and mining districts of the Potomac formation of Maryland, respecting the nature of fossil cycadean trunks were reviewed. One of the more notable conceptions was to the effect that they were a sort of stone plant endowed with a kind of life and the capacity in a marked degree, for *growth*. It was claimed by the owner of one of the trunks, and generally believed in the neighborhood, that when found, in 1830, it was a small stone, and was carried in the apron of Aunt Polly Jones, its discoverer, a distance of not less than two miles, and that since that date it had increased in dimensions until it now weighs 121½ pounds.

Discussed by Prof. Ward relative to geologic and paleontologic character and identification of the specimens; also by Prof. McGee and Dr. J. H. McCormick.

The second communication was entitled 'Seri Stone Art,' by W J McGee. About a score of more or less artificialized pebbles and bowlders were exhibited. Two of these were naturally shaped angular masses of stone, which had been used as metates or nether millstones in simple grinding operations, one so far as to slightly polish the surface, and the other so long as to produce a basin-shape depression nearly an inch deep, in one side; and there were three or four cobblestones which had been used as anvils in simple domestic operations. The greater part of the collection consisted of natural pebbles, more or less completely worn and shaped by use as hammers, manos (or grinding stones), etc.; none of these were differentiated, but all bore marks of use for the various purposes required in the simple domestic art of the Seri Indians. The process of manufacture, or more properly evolution, of the implements, as observed among the Indians and studied in the contents of scores of their rancherias, was described: The Seri matron, requiring implements to crush and grind mesquite beans or to crush bones and sever tendons of turtle or deer, selects a suitable pebble for use as hammer or grinder, with a larger stone which serves as metate or anvil; commonly both are abandoned after a single use, but if the smaller stone is found especially convenient it is preserved for future use, while the larger is used again only if it is near a rancheria; in use the stones are worn, and if improved thereby they are retained and the smaller is carried about by the matron as a part of her domestic paraphernalia; and occasionally a pebble is so satisfactory at the outset and so far improved by gradual reshaping in use that it is completely artificialized, though still used for various purposes, especially grinding and crushing. Eventually the form assumed by such pebbles is a flattened ellipsoid, the sides of which are smoothed and polished by use in grinding, while the ends and perhaps the edges are roughened by battering in the use as a hammer. It was pointed out that even the best worn examples are not the product of purposive manufacture, in accordance with preconceived design, but are simply natural pebbles modified by wear in use, and thus that they cannot be regarded as representing either the neolithic or paleolithic types of many archeologists; and it was suggested that the class be distinguished as protolithic. It was observed that the Seri Indians do not habitually chip stone, and that their stone chipping is limited to the manufacture of arrow-points of a common pattern, and was probably acquired from neighboring tribes, through contact in warfare.

J. H. McCormick,
Secretary.

GEOLOGICAL SOCIETY OF WASHINGTON.

At the 53d meeting of the Geological Society, held on November 25th, 1896, Mr. Arthur Keith, of the United States Geological Survey, gave a resumé of the essential features of a communication presented by him at the previous meeting on the structure of the Cranberry district.

The district lies mainly in the northwestern corner of North Carolina. It extends into Tennessee to the northwest, but the formations under consideration lie entirely within North Carolina. The district consists almost entirely of metamorphic rocks, the great body being gneisses, hornblende schists and allied rocks. All the ordinary features of Appalachian struc-

ture are developed. The rocks are very much deformed, by folding, faulting and metamorphism. The two former are as extreme as in any part of the Appalachians, and the metamorphism is unusually developed. The principal fault, which constitutes the essential feature to be considered, almost encircles an area of Cambrian rocks which are overthrust by granite. The positions and relations of the formations were pointed out on a map of the district. Mr. Keith stated that in the processes of metamorphism the diabases became schists and the granites gneisses. One of the chief features developed is the offsetting of the different formations on opposite sides of a shear zone extending nearly west across the strike.

The great question was, how the deformation of the basement rocks occurred. He mentioned two theories. One is that the sediments above were thrust and defined against the crystallines. This theory he combatted as being untenable. The other theory is that the force that produced the deformation originated in the crystallines and was propagated toward the northwest.

The subject was discussed by Messrs. Hayes and Willis.

Mr. George F. Becker read a paper, entitled 'Some Queries on Rock Differentiation.' The theory now almost generally received ascribes the origin of many igneous rock species to the segregation of a single homogeneous magma under differing physical conditions. Mr. Becker considered segregations due to differences of temperature (Ludwig's or less properly Soret's method) and segregation caused by varying pressure. The active force in these processes is osmotic pressure and this produces segregation by 'molecular flow.' Molecular flow, one case of which is ordinary diffusion, is an exceedingly slow process when the distances involved are considerable; even an aqueous solution of copper sulphate diffuses in sensible quantity to no more than 35 cm. in a year and to ten times this distance in 100 years. Temperature in underground strata diffuses more than 4,000 times as fast as this sulphate. Mr. Becker thinks the process of segregation too slow to produce the observed rock differences. Ludwig's method involves the heating of masses of magma from the top. If such a case were to occur in nature, and this could only be under very exceptional conditions, the temperature would be equalized by conduction through the walls before considerable segregation had had time to take place. Differences of pressure would produce only a very slight segregation even after an infinite time. The separation of a magma into immiscible fractions under changes of temperature was also considered. This involves a superheating of the magma, which seems impossible in the presence of solid rock. It also implies very fluid magmas; too fluid, for example, to hold phenocrysts in suspension. In a superheated magma a decrease of pressure would promote miscibility, not separation. Mr. Becker suspects that the differences between the main rock types is due to the original heterogeneity of the solar nebula, and that many rocks are mere fortuitous mixtures originating at the contacts of primordial masses.

The paper will be printed in the January number of the *American Journal of Science*.

Mr. Whitman Cross was on the program of the meeting for a communication on Landslides in Colorado, but for lack of time the paper was deferred until the next meeting.

W. F. MORSELL.
UNITED STATES GEOLOGICAL SURVEY.

ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA, NOVEMBER 24.

MR. J. H. HAMILTON recorded his recent discovery of what may prove to be an undescribed trap dyke in Fairmont Park, on the line of the newly opened trolley railway, about 175 feet from the Elm Avenue entrance. The trend of the trap is N. by 5°E.

Mr. Richardson stated that he had found, detached, not *in situ*, masses of zoosite at the same locality.

December 1, 1896. Mr. Theodore D. Rand gave an account of a recent trip made to the Wilmington gabbro region, and described the distribution of the quarries as reported by Prof. Chester and as throwing light on the geology of Delaware and Pennsylvania. He referred specially to a mass of the gabbro on the Brandywine Creek, just below the Jessup and Moore paper

mills. In the middle of it is to be observed a distinct schistose structure.

Mr. Edw. Goldsmith described a similar deposit two miles north of that referred to. The masses of the rock which had been mined are so resonant as to merit the name gabbro-phonolite.

Mr. H. A. Pilsbry described a collection of mollusks obtained by himself and Mr. C. W. Johnson in Florida in 1894. Their main object had been to determine the relation of existing mollusks to those of the shell heaps. *Vivipara georgiana* was one of the commonest of the latter, associated with *Unio* and *Ampullaria*. Certain forms of *V. georgiana* from the shell heaps have the spire extremely high. He had called this variety *altior*. The distribution of this form was indicated as being confined to a short stretch of the River St. John's.

He believed that the shell heap *Viviparas* belonged to a race which is now extinct. Another variety of *Vivipara* from the same region was extremely flattened and broadly shouldered. This he had called *limnothauma*. He considered its peculiarity a mechanical result of an acceleration of the reproductive process. This form is also extinct, although some were found more recent than those of the mounds. The characters of both these varieties had appeared during the human period.

Ampullaria depressa was found in the mound three times the size of the living shell. The *Unios* did not vary.

EDW. J. NOLAN,
Secretary.

BIOLOGICAL SOCIETY OF WASHINGTON. 267TH MEETING, SATURDAY, DECEMBER 5TH.

THE evening was devoted to the address of the President, Surgeon General Geo. M. Sternberg, who spoke of 'The Malarial Parasite and other Pathogenic Protozoa.'

F. A. LUCAS,
Secretary.

NORTHWESTERN UNIVERSITY SCIENCE CLUB.

AT the November meeting Prof. Jos. P. Iddings presented the topic 'Petrology as related to other branches of Natural Science.' The outline of his address was as follows:

Petrology deals with theories regarding the origin and formation of rocks, as well as the facts of their existence, and the alteration they may undergo. From geology, as generally defined petrology derives data concerning the occurrence of rock bodies as part of the earth's crust; establishing their formal and quantitative relations, and also their age and time relations. It also derives from geology conceptions of their sources and of the processes of their formation and alteration in general terms. Petrology contributes to geology conceptions of the possible condition of the interior of the earth and of its constitution, and also of the processes of mo. ilar change taking place within it.

From mineralogy petrology derives data cencerning the form, constitution and composition of the mineral components of rocks, which furnish the internal evidence regarding the mode of formation of and the mechanical and chemical changes that take place within rocks. Microscopical study of rocks involves intimate knowledge of geometrical and physical crystallography. While research into the nature and crystallization of igneous rocks necessitates excursions into the realm of molecular physics and chemistry; having to do with the physics of molten rock magmas, and their behavior as solutions in which the dissolved salts are in various molecular conditions, and are capable of diffusion and differentiation. Saturation and crystallization from solution must also be considered, together with the formation of simple and of mixed salts. Chemistry is also called upon to furnish a means of analysis, and to explain processes by which many mineralogical alterations take place. While physics further supplies the laws controlling the transportation and deposition of sediments, and the conceptions of rigidity and plasticity, of stress and strain, of fracturing and shearing that constitute factors of dynamic metamorphism.

To biology petrology is indebted for data regarding the source of material forming certain kinds of rocks. But the processes of growth in the biologic kingdom and those in the mineral realm are strongly contrasted, and the character of an organism and that of a crystal are totally different.

A. R. CROOK,
Secretary.

EVANSTON, ILL.